# PROCEEDINGS

OF THE

# ROYAL SOCIETY OF EDINBURGH.

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ERRATUM.—Page 158, line 12 from top, *delete the words* "(Little Auk)."

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VOL. XX. 1892–93.

## The 110th Session.

### GENERAL STATUTORY MEETING.

*Monday, 28th November* 1892.

The following Council were elected:—

*President.*

Sir DOUGLAS MACLAGAN, M.D., F.R.C.P.E.

*Vice-Presidents.*

Professor Chrystal, LL.D.
Sir Arthur Mitchell, K.C.B., LL.D.
Sir William Turner, M.B., F.R.S.
Professor Copeland, Astronomer-Royal for Scotland.
Professor Jas. Geikie, LL.D., F.R.S.
The Hon. Lord M'Laren, LL.D.

*General Secretary*—Professor P. G. Tait.

*Secretaries to Ordinary Meetings.*

Professor Crum Brown, F.R.S.
John Murray, Esq., LL.D.

*Treasurer*—Adam Gillies Smith, Esq., C.A.

*Curator of Library and Museum*—Alexander Buchan, Esq., M.A., LL.D.

*Ordinary Members of Council.*

Dr R. H. Traquair, F.R.S.
Dr Byrom Bramwell, F.R.C.P.E.
Professor Rutherford, F.R.C.P.E., F.R.S.
Stair Agnew, Esq., C.B.
Rev. J. Sutherland Black, M.A.
Robert Kidston, Esq., F.G.S.
Rev. Professor Flint, D.D.
Dr Johnson Symington, F.R.C.S.E.
Professor John Gibson, Ph.D.
Professor James Blyth, M.A.
Professor D'Arcy W. Thompson.
Professor J. Shield Nicholson.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

His Grace the DUKE of ARGYLL, K.G., K.T., LL.D., D.C.L.
The Right Hon. LORD MONCREIFF of Tulliebole, LL.D.
The Right Hon. LORD KELVIN, LL.D., D.C.L., P.R.S., Foreign Associate of the Institute of France.

PROFESSOR SIR DOUGLAS MACLAGAN, President, in the Chair.

## Chairman's Opening Address.

(Read December 5, 1892.)

Since we last assembled here, and shortly after the last meeting of the session, the British Association met in our city. Though the first meeting of that body was held at York, yet the proposals and counsels which called it into existence, and led to its organisation, emanated from Edinburgh. It originated, in fact, from the suggestions of a distinguished former President of this Society, Sir David Brewster (then Dr Brewster), aided principally by two former secretaries of the Society, Professor Forbes and Sir John Robison. This was acknowledged by the Vice-Chairman of the first meeting at York, who stated in the opening address, "the meeting owes its origin to some distinguished cultivators of science here present," being "proposed by Dr Brewster to the Yorkshire Philosophical Society. The proposal received the most zealous and effective support from Mr Forbes, Mr Robison, and Mr Johnston in Edinburgh, and from Mr Murchison in London."

The first visit of the Association to Edinburgh was in 1834, the fourth year of its existence. The President of that meeting was our former President, Sir Thomas Macdougall Brisbane; Sir David Brewster was a Vice-President, and Professor Forbes and Sir John Robison were the local secretaries. At this meeting the first grant for scientific purposes was made, and amounted only to £20.

In 1850 the Association again visited Edinburgh. Sir David Brewster was President on that occasion. £346 was allowed for scientific purposes.

In 1871 the Association met for the third time in Edinburgh, under the Presidency of Sir William Thomson, now Lord Kelvin. The membership on that occasion was 2463, while the grants for scientific purposes amounted to £1472.

At the last meeting of the Association, Sir Archibald Geikie, in his presidential address, regarding the present year as the centenary of the publication of Hutton's "Theory of the Earth," did justice to the author of that theory, who was one of the most remarkable men that have ever been members of this Society. That theory was originally published in the *Transactions* of this Society (vol. i.), and placed the science of Geology on a new and sure basis. Professor Playfair says in his "Life of Hutton," contributed to the fifth volume of our *Transactions*, "The institution of the Royal Society of Edinburgh had the good effect of calling forth from Dr Hutton the first sketch of a 'Theory of the Earth,' the formation of which had been the great object of his life. . . Several years before the time I am now speaking of, he had completed the great outline of his system, but had communicated it to very few; I believe to none but his friends Dr Black and Mr Clerk of Eldin. . . Yet he was in no haste to publish his theory; for he was one of those who are much more delighted with the contemplation of truth, than with the praise of having discovered it. It might, therefore, have been a long time before he had given anything on the subject to the public had not his zeal for supporting a recent institution (the then newly founded Royal Society of Edinburgh), which he thought of importance to the progress of science in his own country, induced him to come forward, and to communicate to the Royal Society a concise account of his theory of the earth."

I cannot pass from the brief notice just given of this great philosopher without citing what was recently said of him by Sir Archibald Geikie, who thus refers to him: "Hutton died in 1797. . . Men knew not then that a great master had passed away from their midst, who had laid broad and deep the foundations of a new science; that his name would become a household word in after generations, and that pilgrims would come from distant lands to visit the scenes from which he drew his inspiration." Hutton's disciple, Professor Playfair, our former secretary, after long converse with the master, and five years of study, produced his *Illustrations of the Huttonian Theory*. This work, says Sir Archibald Geikie, "for luminous treatment and graceful diction stands still without a rival in English geological literature."

The mantle of Hutton fell upon a younger contemporary, our former President, Sir James Hall of Dunglass, who instituted a remarkable series of researches which laid the foundation of what is styled Experimental Geology. Sir James Hall, acting on Bacon's principle, "non fingendum, nec excogitandum, sed inveniendum quod natura faciat," made the first methodical endeavour to test the truth of geological speculation by appeal to experiment. The value of his work has been recognised not only in this country but on the continent. M. Daubrée, the eminent French geologist, then President of the Academy of Sciences of the Institute of France, who has written a classical work, entitled *Études Synthétiques de Géologie Expérimentale,** in the letter presenting his work to our Society, says :—

INSTITUT DE FRANCE,
PARIS, *le* 19 *Juin* 1879.

MONSIEUR LE PRÉSIDENT,—

Permettez moi de recourir à votre obligeance pour vous prier de faire hommage en mon nom à la "Royal Society of Edinburgh," d'un ouvrage que je viens de publier sous le titre d'Études Synthétiques de Géologie Expérimentale.

C'est sur le sol de l'Écosse que s'est inspiré le génie puissant et fécond de Hutton ; c'est dans les Transactions de votre célèbre Compagnie que James Hall publiait, au commencement de ce siècle deux Mémoires d'une haute importance pour la Géologie Expérimentale ; mon hommage est donc bien motivé.

D'ailleurs comme Président de l'Académie des Sciences de l'Institut de France je saisis cette occasion de payer un tribut d'estime à votre savante Compagnie.

Veuillez agréer, Monsieur le Président, l'assurance de ma haute considération.
A. DAUBRÉE.

Monsieur le Président de la Royal Society at Edinburgh.

In our own days, Sir Archibald Geikie has done much to maintain the renown of this Society for geological research, and has communicated important papers on the geology of Europe and of Scotland, and in particular one of great interest and value on the Volcanic Rocks of the Firth of Forth.

In a paper lately read here, Dr Hatch has shown that the carboniferous volcanic rocks of East Lothian consist of a Lower Basic series, and an Upper Trachitic series, which builds the main

* 2 vols., Paris, 1879.

portion of the Garlton Hills; while Messrs J. Horne and J. J. H. Teale have shown that a group of rocks, which from being extensively developed near Loch Borolan, they propose to call Borolanites, consist of a crystalline granular aggregate of orthoclase and melanite, and belong to the elaeolite-syenite family.

Last session Professor James Geikie communicated to us one of those great geological papers which have done so much credit to our Society, in which the author has propounded views, in many respects striking and original, regarding the evidences of a succession of ice ages. In this paper on the glacial succession in Europe, Professor Geikie gives a general review of the evidence which has accumulated during late years. He is of opinion that the climatic changes of the glacial period were more numerous and complete than is generally supposed. According to him, five cold or glacial epochs have alternated with four genial or interglacial epochs. The first cold epoch and succeeding genial period supervened in Pliocene times. Thereafter followed the second glacial epoch, during which the cold conditions were most intense. This was indeed the climax of the glacial period, when a great ice-sheet flowed south into Saxony—the British and Scandinavian mer de glace being confluent in the North Sea basin. Again the climate changed, and genial conditions, comparable to those of the present, obtained in Northern and North-Western Europe. This second interglacial stage was followed as before by a glacial relapse, when the British and Scandinavian ice sheets again coalesced, but the northern mer de glace did not flow further south on the continent than the valley of the Elbe. Once more glacial conditions disappeared, and were succeeded by the temperate climate of a third interglacial epoch. Erelong, however, the temperature again fell, and the fourth genial epoch ensued. Great snow-fields and glaciers then came into existence in the mountainous districts of the British Islands, while Norway, Sweden, and Finland were largely ice-clad—a mighty glacier occupying the basin of the Baltic, and invading the low grounds of North Germany, Schleswig, Holstein, and Denmark. The British and Scandinavian ice, however, did not again become confluent,—the mer de glace of Norway calved its icebergs at the mouths of the great fiords all along the west coast of that country. Eventually these glacial conditions passed away, and a temperate climate followed, during the prevalence of which a temperate flora and fauna clothed and peopled northern and north-western Europe. Finally another glacial relapse —the fifth—took place, when small local glaciers appeared in the higher mountain valleys of this and other temperate latitudes. It was shown that this remarkable alternation of cold and genial climates characterised central as well as northern Europe—the glacial and interglacial deposits of the Alps telling precisely the same tale.

Leaving the subject of geology—on which I have dwelt, perhaps, too long, in consequence of this year being regarded as the centenary of Hutton, and from that science being so much identified with the reputation of this Society—I proceed to advert to a remarkable celestial event, which will make this year memorable in the annals of astronomy,—the appearance of a new star, known as Nova Aurigæ. This star was discovered by that able observer the Rev. Thomas D. Anderson, D.Sc. in classical philology, a citizen of Edinburgh, who communicated his discovery, which was destined to make a sensation in the astronomical world, to Dr Copeland, our Astronomer-Royal, in an anonymous postcard. Dr Anderson stated at a meeting of this Society that his discovery was made with the aid of a pocket telescope, magnifying not more than ten times, and with a small star-atlas. Papers by our Astronomer-Royal, Dr Copeland, and his assistant, Dr Becker, in reference to this celestial visitant, have been read during the past session, and these two astronomers have given an elaborate drawing of its spectrum.

Dr Anderson also discovered, independently, a new comet in the girdle of Andromeda, but he was anticipated by some hours in this discovery by Mr Holmes, an English amateur astronomer, and the comet will accordingly bear the name of the English observer.

Mr John Aitken, of Falkirk, like Dr Anderson, has done much valuable scientific work with but limited instrumental means. Last session he contributed a paper on "The number of Dust Particles in the Atmosphere, and on the Relation between the amount of Dust and Meteorological Phenomena," being the last of his interesting communications on this subject.

Our General Secretary gave us a continuation of his papers on the Kinetic Theory of Gases, a recondite subject requiring for its successful treatment profound thought and the resources of the higher mathematical analysis; whilst Professor Knott dealt with some interesting phenomena of circular magnetisation.

Dr Muir, Professor Tait, and Lord M'Laren contributed new and simpler solutions, obtained by different methods, of what is known as Sylvester's Elimination Problem, from its being first solved by that mathematician.

Sir William Turner, continuing his valuable papers on the Cetacea, favoured us last session with a communication on the Anatomy of the Rorqual; and Mr Malcolm Laurie, though only commencing his career as a naturalist, has given a paper of much local interest "On some Eurypterid Remains of the Upper Silurian Rocks of the Pentland Hills."

Professor Blackie's paper shows how the latest phases of literary style in Greece approximate more and more to the purity of the ancient classical language.

Dr Felkin favoured us with a long and interesting account from personal observations of the Wanyoro Tribe, which has a population of two and a half millions, and is in close proximity to Uganda.

In a remarkable paper Dr Noël Paton has reinvestigated the question of how the auriculo-ventricular valves are closed, and how they prevent the regurgitation of blood into the auricles. He thinks it possible that these valves might fulfil their function of preventing regurgitation in just as perfect a manner and without the severe strain which, according to the presently accepted theory, they must sustain with each systole, if, instead of being raised to form a horizontal septum, their cusps are simply applied face to face with one another.

During the past session twenty-one candidates for admission have been elected Fellows of the Society; and during the same period eleven of our Fellows have died. I shall here advert briefly to the career of each of the deceased.

Sir James Brunlees was born at Kelso in 1816. He was educated at the University of Edinburgh, and subsequently in the Scottish capital had a considerable practice as a surveyor. In 1838 he became assistant engineer to Mr Alexander Adie in the construction of the Bolton and Preston Railway, one of the first lines laid down in this country. Between 1844 and 1850, under Sir John

Hawkshaw, he carried out the extensive works of the Lancashire and Yorkshire Railway system. In 1850 he was engaged on the construction of the Londonderry and Coleraine Railway, and two years later undertook the difficult works of the Ulverston and Lancaster Railway across Morecambe Bay. From that time onward his principal works at home included the Solway Junction Railway, with its viaduct a mile and a quarter long across the Solway Firth; the Clifton Extension Railway; the Mersey Tunnel Railway; and the Avonmouth, King's Lynn, and Whitehaven Docks. Abroad he constructed the Central Uruguay and Bolivar Railway, the Minas and Rio Railway, and the Porto Allegre Railway, and received from the Emperor of Brazil the decoration of the Rose. He was one of the past Presidents of the Institution of Civil Engineers. He was elected a Fellow of this Society in 1878, and died on 2nd June 1892.

Professor WILLIAM DITTMAR was born in Umstadt, near Darmstadt, Germany, in 1833. He entered in 1857 the laboratory of Bunsen, and became the assistant of that celebrated chemist. Whilst holding this appointment, Professor Roscoe, who had made his acquaintance, invited him to become his private assistant. After Sir Henry Roscoe's appointment to the Chair of Chemistry in Owens College, Mr Dittmar accompanied him thither, and only left Manchester to become, in 1861, Dr Lyon Playfair's chief laboratory assistant. In 1869 he returned to Germany, and during the next three years he acted as a "Privat Docent" and Lecturer at Poppelsdorff. He returned to Scotland in 1872 to hold under Professor Crum Brown the same post which he had held under Sir Lyon Playfair. On being offered the newly-instituted Chair of Practical Chemistry in Owens College, he removed to Manchester, but returned to Scotland on being appointed to the Chair of Chemistry in Anderson's College, Glasgow.

Critics are united in recognising the thoroughness, great variety, and true scientific worth of all he has done. He was awarded the Graham Medal for his difficult and successful investigation of the "Gravimetric Composition of Water." He is best known by his contribution to the publications of the "Challenger" Expedition. To this work he devoted three years of incessant work. He also wrote numerous and voluminous articles for the cyclopædias of Britain and the Continent.

He was a man of genial disposition and unaffected in his manners. He received the degree of LL.D. from the Universities of Edinburgh and Glasgow, was elected a Fellow of this Society in 1863 and a Fellow of the Royal Society of London in 1882. He died on the 9th of February 1892.

Alexander Forbes Irvine of Drum.—It is with feelings of the deepest regret, which will be shared by many here present, that I advert to the decease of Sheriff Forbes Irvine, whose stately and courteous presence and kindly bearing, which used to grace the meetings of Council and the Society, will be seen amongst us no more. An admirable obituary notice has been written of him, giving an account of his historic ancestry, for our *Proceedings* by Sheriff Æneas Mackay, to which it will be sufficient for me here to refer.

Dr Alexander Keiller.—Of this distinguished and amiable physician, who was esteemed both by the profession to which he belonged and by his fellow-citizens, a biography has been written for our *Proceedings* by Dr T. A. G. Balfour.

Sir George Husband Baird Macleod.—As a full obituary notice of this distinguished surgeon has been written for our *Proceedings* by his son, the Rev. W. H. Macleod, minister of Buchanan, I shall merely refer to it for the main incidents in the career of the deceased.

Thomas Nelson.—As a biographical notice of him has been written for our *Proceedings* by Dr W. Scott Dalgleish, I must refer to it for the incidents of his life. I shall only say that he was a man of great insight and forethought, and of promptitude in action. These qualities, which belonged to him and the other members of the Nelson house, led to the high style of work sent out by them to the reading and educational world. He was a man of kindly disposition, and bequeathed upwards of £50,000 for the promotion of benevolent schemes in Edinburgh. He was elected a Fellow of this Society in 1866.

Dr William Forbes Skene.—He was born at Inverie on 7th June 1809. His father was James Skene of Rubislaw, and his mother a daughter of Sir William Forbes, Bart. of Pitsligo. Educated at the High School of Edinburgh, he afterwards studied at the Universities of St Andrews and Edinburgh. He was admitted a Writer to the Signet in 1831, and was subsequently appointed

Depute-Clerk of Session. For about forty years he remained the head of the prominent legal firm of Skene, Edwards, & Garson, and was one of the Directors of the Commercial Bank. Although Dr Skene enjoyed a high reputation as a lawyer, he is more widely known as a historian, archæologist, and scholar. The first work which he published was entitled *The Highlanders of Scotland: their Origin, History, and Antiquities.* This work, which appeared in 1837, was awarded the prize of the Highland Society, and is considered to show rare critical acumen and ripe Celtic scholarship. He edited *The Dean of Lismore's Book*, and various other works relating to the early history of Scotland. In 1876–80 he produced his *magnum opus*, entitled *Celtic Scotland: a History of Ancient Alban.* For over forty years he had been collecting material for this work, which is recognised as a monument to the learning, industry, and ability of its author. In 1865 he received from the University of Edinburgh the degree of LL.D., and in 1879 the degree of D.C.L. from the University of Oxford. On the death of Dr John Hill Burton in 1881, Mr Skene was appointed Her Majesty's Historiographer for Scotland. I must advert, however briefly, to his labours as a philanthropist. In 1846 the potato crop failed in the Highlands, and a Relief Committee was formed, which from 1846 to 1850 raised and distributed a sum of about a quarter of a million in relief works, such as the making of roads, bridges, and piers, by which many districts formerly inaccessible were opened up to traffic. Mr Skene was appointed secretary to this committee, and devoted the greater part of his time to the work of relief. He was admitted a Fellow of this Society in 1859, and died on 29th August 1892, at the age of 84.

Lord TENNYSON.—Of Lord Tennyson, an Honorary Fellow, whose career and works are so well known, I shall only say that his poetry is characterised by stateliness, depth, and power, and his versification is lofty, graceful, and sonorous. He is a master of English song, whom only Shakespeare and Milton have surpassed. He was elected an Honorary Fellow of our Society in 1864.

Professor JAMES THOMSON, F.R.S., LL.D., who held successively the Chairs of Engineering in Queen's College, Belfast, and in the University of Glasgow, was elected a Fellow of this Society in 1875. I cannot help adverting to his singular simplicity of character and his unfailing courtesy, but it is unnecessary for me to enlarge upon his attainments or his eminence as a man of science, as a notice of him will be contributed to our *Proceedings* by our Secretary, Professor Tait.

John K. Watson was born in the year 1818. He held the responsible and onerous position of Manager and Treasurer of the Edinburgh Gas-Light Company for many years. His father had previously been manager for the Company. He took a deep interest in the proceedings of this Society, and was a man of great business capacity and of very genial manners. He was elected a Fellow of this Society in 1866, and died on 17th November 1891.

Sir Daniel Wilson.—Daniel Wilson was born in Edinburgh in 1816. In due course he was sent to the High School, and while prosecuting his studies there, indulged in rambles in and about Edinburgh, to Cramond, Roslin, Preston Tower, and other places, and such excursions helped to form his tastes for antiquarian pursuits. From the High School he went to the University. Whilst there he took a prominent part in founding a debating society, which received the name of The Zetalethic, and which dealt with such problems as the comparative happiness of married and single life, and the duty of resisting tyrants.

After leaving college, he threw himself with great earnestness into antiquarian pursuits. In 1847 he published his *Memorials of Edinburgh in the Olden Time*,—a work which gathers up for us the history, traditions, and life of old Edinburgh, and by its illustrations, many of them from his own hand, fixes the aspect of the old town. He was for some years secretary to the Scottish Society of Antiquaries.

More important than his *Memorials* was his great work on the *Archæology and Prehistoric Annals of Scotland*, which appeared in 1851. This work will probably secure for him his most enduring fame. He also wrote on Cromwell, on "Caliban, or the Missing Link," on Chatterton, and on "Lefthandedness," besides producing a volume of poems. In 1853 he was appointed Professor of History and English Literature in the University of Toronto. In 1881 he succeeded Dr M'Caul as President of the Toronto University, and in 1887 received the dignity of knighthood. He was elected a Fellow of this Society in 1875, and died on 8th August 1892.

JOHN COUCH ADAMS.—As the incidents in the career of this extraordinary man have been given in so many biographical notices that have appeared in Europe and America, and as an obituary of him has been prepared for our *Proceedings* by the Astronomer-Royal for Scotland, I shall merely say that he was elected a Fellow of our Society in 1849, and that a valuable collection of his papers on the Lunar Theory and other astronomical subjects, and also on the Theory of Numbers, was lately presented to the Library of this Society by his widow, Mrs Adams.

Sir GEORGE B. AIRY.—As an obituary notice has been prepared of him for our *Proceedings* by the Astronomer-Royal for Scotland, and will be read in due course, it is unnecessary for me on this occasion to enter into the details of his life. He was elected a Fellow of this Society in 1835.

Since last meeting I am happy to announce that the Society has obtained from the Board of Manufactures a new room, which was previously occupied by the Society of Antiquaries, and which will for some time afford accommodation to the additions of books which are coming in to our library so fast, and in such large quantity, from almost all the scientific societies of the world.

The Society was lately invited by the University of Padua to send a representative to take part in the celebration of the tercentenary of the appointment of Galileo to the Professorship of Mathematics in that University—the date of his first lecture being the 7th December 1592. The letter of invitation says—"Illo enim die, Anno MDXCII. summus acerrimusque investigator legum quibus caelestium terrestriumque rerum natura continetur, hic cathedram ascendit, eamque voce suâ immortalitati commendavit. . . . In spe sum, fore, doctissimi viri, ut, si quis vestrum adsit, et feriis nostris decus augeatur et apertius fiat, quanti sit apud homines veritatis studiosos gloria viri, qui certam rerum experiendarum viam ac rationem invenit atque constituit." Signed by the Rector CAROLUS F. FERRARIS, xxvi. Septem. MDCCCXCII.

To this invitation the Council authorised the following reply to be presented to the University, through Professor Cremona of Rome, who was appointed the Society's representative:—

Rectori Magnifico et Doctissimo Senatui Universitatis Patavinæ.

VIRI ILLUSTRISSIMI,—

Societas Regia Edinensis commemorationem illius diei, quo Galilæus Galilæus cathedram ascendit Universitatis Patavinæ, concelebrare vult, et reverentiam præstare illi summo viro, qui impavidus, magno veritatis percussus amore, quamvis obstante altissimâ potestate, verum mundi systema ingeniosis observationibus confirmavit, atque certam et fructuosam naturæ indagandæ methodum invenit atque illustravit.

Nos illustri Collegæ nostro Honorario Professori Cremonæ vicem Societatis repræsentandæ mandavimus.

DOUGLAS MACLAGAN, *Præses.*
P. G. TAIT, *a Secretis.*

Apud Aulam Societatis
Edinburgi, iii. Cal. Dec. 1892.

A very general desire having been felt to give effect in a substantial and permanent form to the high appreciation entertained of Professor Tait's services to the Society, it was considered that the most appropriate way of doing this would be to have his portrait painted, and placed in the Society's meeting-room, along with the portraits of the eminent men—Presidents and Secretaries—who have shed lustre on the Society. The portrait is the work of Sir George Reid, *P.*R.S.A., and will be presented to-night.

## On the Madder-Staining of Dentine. By W. G. Aitchison Robertson, M.D., D.Sc., F.R.C.P.E., Physiological Laboratory, University of Edinburgh. (With a Plate.)

(Read December 19, 1892.)

While working at the histology of the tooth, it occurred to me that some light might be thrown on the vexed question as to the manner in which dentine grows, by observing what parts of a tooth are stained when an animal is fed on madder mixed with its food. In order to investigate this I examined the persistently growing teeth in three young rabbits, all of the same litter, which had been fed on madder (*Rubia tinctorum*) for two weeks. The first rabbit was killed immediately after being thus fed; the second was killed after feeding for *two* weeks on ordinary diet subsequent to the madder food; while the third was killed *three* weeks after feeding with madder had been discontinued.

### I. Young Rabbit killed after Feeding for two weeks on Food with Madder.

(A.) *Lower Incisor.* Length, 14 mm.; greatest breadth, 2 mm.

(1) *External appearance.* The greater part of each tooth is stained a beautiful light lake colour. This grows lighter, and fades off entirely towards the free extremity of the tooth, leaving the upper quarter quite unstained and white. The staining is most marked towards the root and in the natural furrows (fig. 1, *a*).

(2) *On section.* The upper third of the tooth is almost quite unstained, except at the centre in the position of the obliterated pulp-cavity. In this position the staining extends up to the free masticatory surface of the tooth as a narrow band on each side of the dark line representing the obliterated pulp-cavity. Although the whole of the lower two-thirds is stained, the dentine in immediate contact with the pulp is stained more deeply than that situated further out (fig. 1, *b*).

(B.) *Molar Teeth of Lower Jaw.*

*First Premolar Tooth;* length, 8 mm.; greatest breadth, 1·5 mm.

(1) *External appearance.* The upper half of the tooth is unstained, but towards the root the colour gradually deepens and reaches its greatest intensity at the free margin of the root (fig. 1, *c*).

(2) *On section.* Here again we see that the upper half is free of stain, except at the centre of the tooth, where the staining extends, as in the incisor, up to the free grinding surface. It also shows that the dentine which immediately surrounds the pulp takes on a deeper stain (fig. 1, *d*).

The explanation of the staining of these teeth is obvious after what I have already shown in my paper on the growth of dentine,* and corroborates the conclusions at which I had already arrived. The unstained part of the tooth had existed before the animal was fed on madder. All the stained parts of the tooth have grown since the madder feeding was commenced. The portion showing the gradual transition between the unstained and stained parts of the tooth marks the lowest part of the tooth at the time when the madder was first given, and therefore the more deeply stained part below this had grown after that time.

It is seen in the section that the staining reaches the crown, but only at the core or centre of the tooth. This coloured new dentine had been developed on the inner surface of the old during the fortnight of feeding with madder. At the apex of the pulp-cavity the staining is deepest, because in that situation the oldest part of the tooth is found, and consequently the deposit of madder-stained dentine has been going on longest there.

These specimens show that the incisors increase in length relatively much more than the molars. Only about one quarter of the length of the incisor tooth is unstained, while in the premolar nearly one-half is unstained. The stained dentine was developed during the fortnight of madder feeding: in that time as much as three-fourths of the length of the incisor had grown, but in the premolar only a half.

* "On the Relation of Nerves to Odontoblasts, and on the Growth of Dentine," *Trans. Royal Society of Edinburgh*, vol. xxxvi., pt. ii., p. 321 (1891).

### II. Young Rabbit fed on Madder for two weeks, and then on Ordinary Food for other two weeks.

(A.) *Lower Incisor.* Length, 19 mm.; greatest breadth, 2 mm.

(1) *External appearance.* The whole of the tooth was stained of a light lake colour, with the exception of the lower fourth, which was unstained and white. The staining is more marked towards the free extremity, and fades off as it approaches the lower end (fig. 2, *a*).

(2) *On section.* The greater part of the cut surface is stained, but there is a narrow line of unstained white dentine immediately around the pulp-cavity, and this unstained portion is continued upwards at the centre of the tooth to its free masticatory surface. The unstained band occupies the same position as the stained band which reached the free surface in the corresponding tooth of the preceding rabbit. The dentine on the concave side of the tooth is more deeply stained than that on the convex side. The lower fourth of the tooth is unstained (fig. 2, *b*).

(B.) *First Premolar.* Length, 10·5 mm.; greatest breadth, 3 mm.

(1) *External appearance.* Nearly the whole tooth except the lower third is stained. The staining at the upper extremity or crown is very faint. The lake colour is deepest about the middle of the tooth, and gradually fades off towards either extremity (fig. 2, *c*).

(2) *On section.* Immediately around the pulp-cavity there is a narrow band of unstained dentine. This is continued up for a considerable distance in the core of the tooth, in the position of the obliterated pulp-cavity. External to this the rest of the tooth is coloured, with the exception of the lower third, and a small part at the free extremity surrounding the central stained core (fig. 2, *d*).

The lower unstained portion of the tooth is that developed from what I have termed the "formative ring," viz., circle of odontoblasts that are always lowest in position, during the interval between the cessation of madder feeding and the death of the animal. The narrow strip of unstained dentine around the pulp-cavity is the new dentine which had been deposited during the same period on the inner surface of the preformed dentine. In the incisor tooth it extends upwards to the free surface, the upper part of the pulp-cavity becoming obliterated by the deposit of this fresh dentine. By examining figs. 1 and 2, one can readily see all the dentine that was formed during the period when madder was given. The small portion remaining unstained at the crown of the premolar is a portion of the old tooth not yet worn away. It was the lower part of the tooth previous to the administration of the madder. The axial portion of the upper extremity of this tooth is, however, stained, and we find the explanation of this by comparing it with the condition of the teeth in Rabbit No. I. In them the coloration extends from the newly-formed dentine inclosing the pulp-cavity up to the free surface of the crown, through that which had obliterated the upper part of the cavity. But in the premolar of Rabbit No. II. only the lower end of this axial band of coloured dentine is seen not yet worn away.

The teeth of this second rabbit again exhibit the rapid growth of the incisor compared with that of the molar teeth. In the former the staining extends up to the crown, while in the latter the crown is still unstained, because composed of dentine developed before the madder was given. The deeper staining of the dentine on the concave side of the incisor may be due to the fact that the inner surface of this tooth grows more rapidly than the outer, to compensate its being worn down more rapidly. It thus requires more nutrition, and will therefore absorb more of the circulating madder.

### III. Rabbit fed on Madder for two weeks, then on Ordinary Food for three weeks.

(A.) *Lower Incisor.* Length, 22 mm.; greatest breadth, 2·5 mm.

(1) *External appearance.* Nearly the upper half of the tooth was stained, and the staining was deepest at its free extremity. Only the edges of the grinding surface of the tooth were stained pink, while the central part was unstained (fig. 3, *a*).

(2) *On section.* Nearly all the dentine was found unstained; only that situated at the periphery of the tooth and at its upper part had taken on the madder-stain (fig. 3, *b*).

(B.) *First Premolar Tooth.* Length, 12 mm.; greatest breadth, 3 mm.

(1) *External appearance.* Only the upper third of the tooth was

stained, and the coloration was deepest at the free extremity (fig. 3, *c*).

(2) *On section.* The greater part of the tooth was white. There was merely a thin madder-stained line running down each lateral border of the tooth from its free extremity for about one-third of its length (fig. 3, *d*).

The appearances found in the teeth of this last rabbit were only further stages of what we have already seen in the teeth of Rabbits I. and II. Almost the whole of the old stained dentine had been worn away, and what remained of it was merely a thin cylinder at the upper part of the tooth, which now consisted almost entirely of new dentine which had been formed since the madder was stopped. The dentine situated between the lower limit of the madder-stain and the base of the tooth was that produced by the "formative ring" during the post-madder period. The large amount of unstained dentine surrounding the pulp-cavity and extending up to the free surface had also been deposited on the inner surface of the old in the same space of time.

| | Incisor. | | Premolar. | |
|---|---|---|---|---|
| Rabbit. | Length. mm. | Breadth. mm. | Length. mm. | Breadth. mm. |
| I. | 14 | 2 | 8 | 1·5 |
| II. | 19 | 2 | 10·5 | 3 |
| III. | 22 | 2·5 | 12 | 3 |

From the examination of madder-stained teeth we thus derive much information regarding the manner in which persistently growing teeth increase or maintain their adult size. It clearly shows that as the teeth increase in length so do they in breadth. I have shown that this increase in breadth always commences at the root, and therefore the breadth is always greatest in this situation. In three weeks (compare I. and II.) the breadth of the incisor tooth had increased by one-fourth, while in the same time the premolar had become twice as broad as before. We see also that the teeth increase in length by the addition of new dentine to the lower end, and that this growth more than compensates the loss from wearing down the crown in grinding. The increase in length of the incisor teeth being here 5 mm. in fourteen days and 8 mm. in twenty-one days is readily seen with the unaided eye, since the newly-formed dentine is white while the old is pink. The premolar had also gained 4 mm. in length in twenty-one days. These two processes, gradual increase in breadth and in length, go on both of them at the same time, and both are due to the action of the odontoblasts of the "formative ring."

The deposition of new dentine on the inner surface of that already formed is well seen in these madder-stained teeth. The recent dentine appears as a white lining to the tooth in Rabbit No. II.; while in No. III. it is a white lining so thick that it forms the larger part of the tooth. In both cases the white lining is thickest at the apex of the pulp-cavity, and thins off towards the root. The dentine at the upper part of the pulp-cavity has, during the persistent growth of the tooth, travelled up from the root, and during its passage there has been a continual deposition of new dentine on its inner surface. This accounts for the large amount of new dentine at the upper end of the pulp-cavity and the consequent obliteration of this cavity in the crown. We can now explain why it is that in Rabbit No. III. the stained dentine forms only a thin hollow cylinder at the upper part of the tooth. The dentine formed during the period of madder feeding had been subsequently so worn away that barely half the tooth is stained, the lower part having been formed after the madder was stopped. The new white dentine had then been deposited on the inner surface of the old stained dentine, and had replaced it as it was worn away. Thus we have the thin coloured cylinder alone left; and had the animal been allowed to live a week or two longer, this would also have been worn away, and the whole tooth would have regained its normal white colour.

I conclude by tendering my best thanks to Professor Rutherford, in whose laboratory this work was carried on, and who has kindly revised this paper. Both he and Dr J. Berry Haycraft personally supervised my work, and rendered me every assistance in the execution of it.

*Figures of Madder-Stained Teeth.*

Fig. 1. Teeth of young rabbit fed on madder for two weeks and then killed.

*a.* Incisor tooth—surface view.

*b.* Incisor tooth—in section.

*c.* First premolar—surface view.

*d.* First premolar—in section.

Fig. 2. Teeth of young rabbit fed for two weeks on madder, then on ordinary food for two weeks, and then killed.

*a.* Incisor tooth—external surface.

*b.* Incisor tooth—in section.

*c.* First premolar—external view.

*d.* First premolar—in section.

Fig. 3. Teeth of young rabbit fed for two weeks on madder food, then for three weeks on ordinary food, and then killed.

*a.* Lower incisor—surface view.

*b.* Lower incisor—in section.

*c.* First premolar—surface view.

*d.* First premolar—in section.

DR. W. G. AITCHISON ROBERTSON ON MADDER-STAINING OF DENTINE.

Fig. 1

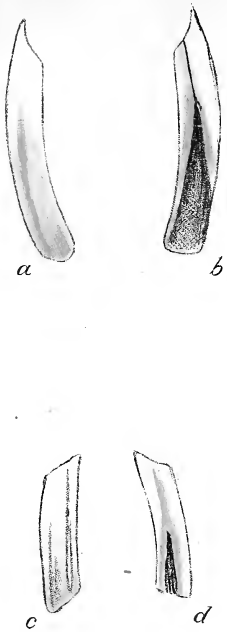



Fig. 2

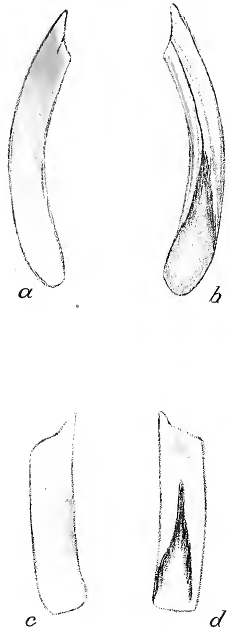
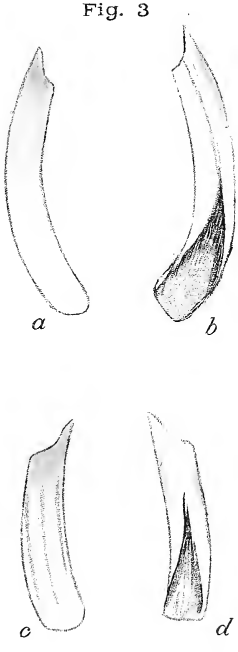



Fig. 3



## On the Comparative Histology and Physiology of the Spleen. By Arthur J. Whiting, M.D.

(Read January 10, 1893.)

(*Abstract.*)

The principal subject of the author's research has been the comparative histology of the spleen in twenty-two different kinds of animals, embracing all the typical vertebrates from the fish to Man.

The capsule of a typical mammalian spleen, such as that of the cat, consists of two layers, the outer composed of ordinary fibrous tissue, the inner of nonstriped muscle. The splenic artery and vein, as they enter the organ at the hilum, are enclosed in a sheath formed by an inflexion of the capsule, and, like it, consisting of a fibrous and a muscular layer, the fibrous layer being next the vessels, and the muscular layer external to it, adjoining the pulp. This perivascular sheath may be termed the hilar sheath of the vessels. The muscular layer of the capsule sends inwards a ramifying system of cord-like trabeculæ consisting of non-striped muscle that join the muscular layer of the hilar sheath and form a contractile framework for the organ.

The principal variation in the supporting framework of the spleen in different classes of animals is in two directions—in the degree of its muscularity, and in the degree of development of the trabecular system. Non-striped muscle was found in the framework of all the spleens examined. At its minimum in fishes, it gradually increases in amount as the scale is ascended through the amphibia, the reptiles, the birds, to the mammals, and reaches its maximum in the ungulata or in the carnivora.

A trabecular system proper was found in mammals alone, most highly developed in the carnivora, and least in the cetacea.

After the artery and vein have run together for a short distance into the interior of the spleen invested by the hilar sheath, they separate, the veins branch off along the trabeculæ, while the arteries continue their course branching dichotomously within the hilar sheath. But after separating from the vein they become enveloped

in a sheath of lymph follicular tissue which is between the hilar sheath and the vessel. The arterioles are ensheathed doubly in a similar manner, but the adenoid sheath varies in thickness owing to the development of nodular swellings,—the Malpighian bodies or splenic follicles at irregular intervals. The follicles are enclosed in a contractile envelope chiefly composed of spindle-shaped muscular fibres derived from the hilar sheath.

In all mammals, with possibly one exception—the guinea-pig, in some birds, and in some cartilaginous fishes, the lymph follicular sheath of the arterioles is developed into splenic follicles. But in osseous fishes, in the amphibia, in the chelonia, and in some birds, the lymph follicular sheath shows no special development.

Lymphoid cells occur everywhere in the adenoid sheath, but most numerously in the follicles, which mainly consist of them. In the follicles the cells are divisible into two groups,—a central area of larger cells or leucoblasts forming the germinal centre of Flemming, and a peripheral area of small lymphoid cells resulting from division of those in the germinal centre. The lymphoid cells are extruded into the surrounding pulp partly by the centrifugal pressure of the formation of daughter cells, and partly by the compression of the follicles by their muscular covering. Multinucleated giant cells are sometimes found within the follicles, especially in the spleen of young mammals.

The terminal portions of the splenic arterioles are invested by a continuous sheath, consisting of a homogeneous ground substance containing a few lymphoid cells, as in the osseous fishes, in the chelonia, and in some birds; or by such a sheath developed into a nodose circumscribed swelling containing a few lymphoid cells and concentrically arranged spindle cells—probably muscular. These bodies were termed splenic "ellipsoids" by W. Müller, and "capillary sheaths" by Schweigger-Seidel. The axial vessel usually divides in the substance of the ellipsoid, and leaves it as thin-walled veins that open into the spaces of the pulp. It gives off capillary channels that anastomose with each other, and open into a blood sinus that surrounds the ellipsoid. This blood sinus communicates with adjacent ellipsoidal sinuses and splenic veins, but apparently not with the spaces in the pulp, nor with the emergent vessel of another ellipsoid.

The reticulum of the splenic pulp is formed by the anastomosing expanded processes of nucleated plate-like cells, and is continuous with the supporting framework of the spleen.

The cellular elements of the pulp are of four principal kinds—lymphoid cells, protoplasmic corpuscles, cells containing pigment—probably phagocytes, and special cells of the nature of giant cells.

*The lymphoid cells* are nearly all small and uninucleated, and are probably derived from the follicles. Multinucleated lymphoid cells are few in number, and are probably derived from the blood.

*The protoplasmic corpuscles* are of three kinds—coarsely granular, hyaline (and these are either erythroblasts or nucleated red cells), and eosinophilous cells.

*Giant cells* occur in the spleen of mammals during late intra-uterine and early extra-uterine life, and usually in large numbers. They occur during adult life, in considerable numbers, in the spleen of some small mammals, as the mouse, rat, and hedgehog. Apparently homologous cells were found in the spleen of the tortoise. They are associated with numerous erythroblasts and nucleated red cells in the pulp and frequently with active karyokinesis in the follicles, and therefore seem to be a concomitant of the hæmatopoietic activity of the spleen. They usually possess a large central nucleus that gives off pyriform buds which become isolated. The giant cells were never seen to contain red corpuscles, as has been stated by those who regard them as destroyers of red corpuscles. The author believes them not to be phagocytes. They mostly occur in the pulp, but may sometimes be seen in the radicles of the splenic vein. Bizzozero and Salvioli have shown that when dogs and guinea-pigs are rendered anæmic by copious blood-letting, the spleen takes an active part in the production of nucleated red-blood cells. If the multinucleated giant cells are phagocytes, one would scarcely expect their number to be increased when the spleen is called upon to increase its blood-forming activity. The author examined the spleens of five dogs that had been rendered anæmic by loss of blood (three of them kindly given to him by Dr Robert Muir), and in all of them there was a great increase in the number of giant cells found, with large numbers of erythroblasts and nucleated red cells. On the other hand, it has been ascertained by Neumann and Freyer and by Bizzozero and Salvioli that in the rabbit artificial anæmia does not stimulate the spleen to hæmopoietic activity. The author confirms this observation. He found that the spleen of a rabbit that had been copiously bled contained few erythroblasts, and nucleated red cells, and almost no giant cells. It is believed by some investigators that in mammals, after embryonic life, the nucleus of the young red-blood cell becomes extruded, so that the corpuscle becomes non-nucleated, and as such enters the circulating blood. Omer van der Stricht is of this opinion; he believes that the extended nuclei are devoured by giant cells, and that the numerous nuclei in giant cells in blood-forming organs are such nuclei undergoing disintegration. The author is opposed to this view, because he has traced the development of the nuclei, but reserves his opinion regarding the significance of the remarkable increase in the number of multinucleated giant cells in the spleen when it is actively producing blood corpuscles in early life and after artificial anæmia.

## Induction through Air and Water at Great Distances without the use of Parallel Wires. By Charles A. Stevenson, B.Sc., F.R.S.E., M.Inst. C.E. (With a Plate.)

(Read January 30, 1893.)

At the beginning of last year I proposed that a cable might be laid down in the sea, and, by changing the electric state of the cable, that vessels passing near or over it might be able by means of a detector on board to discover that they were in its vicinity, and hence to locate their position. Some experiments showed that the method was feasible, and that water offered no insurmountable difficulty; and having been assured that no known instrument could detect the currents that could, with our present machinery, be passed through a submarine cable, a series of exhaustive experiments were made, with the result that I have constructed two such instruments that will act through over 30 fathoms of water, and I have been unable to discover their like.

The first instrument now brought before the Society is a coil of uninsulated copper-wire rope, or other water connection, dipping into the water at the bow of a boat, and a similar water connection at the stern (fig. 1). If these are joined by a wire with a telephone on the circuit it will be found that even without an induction coil or other arrangement to magnify the effect a very sensitive instrument is produced, and that when the wires from bow to stern of the boat are at *right angles*, or nearly so, to a cable laid in the water at some distance from it, the sounds produced by a magneto-electric machine connected to *one* pole of the machine are audible in the telephone. If the water connections are equidistant from the cable, as they would be if the boat were immediately over the top of it or lying broadside on, no sound is, of course, heard.

The action takes place when the coils in the water are insulated. The cable also may be insulated or uninsulated, as is shown by the diagrams (fig. 1), showing effects with different cables, these experiments being made with a small medical battery 4″ in length. The action, of course, is similar with an induction coil, and will also act if the potential of the cable is changed and is then kept so. In this case only the leakage has to be supplied. The apparatus appears to act electrostatically on the principle of a voltmeter detecting differences of potential produced by the charging and discharging of the cable, or when the cable is kept charged, by the difference of potential near and at a distance from a conductor. It is injuriously affected by the increase of conductivity of the water between the water connections due to increase of salinity, but the law by which it varies has not been determined. This loss of efficiency in sea water is, of course, counteracted by the insulation of the water connection, or by introducing an equivalent resistance.

With the coils separated 10 feet (at the bow and stern of a small boat) and an insulated wire 400 feet in length laid through a small lake of brackish water $2\frac{1}{2}$ fathoms in depth, the alternations produced by the bobbins of $\frac{3}{5}$ths of one of De Meritens' magneto-electric machines used at the Isle of May Lighthouse, and maintaining 80 volts at its terminals, was perfectly distinct at the end of the loch 340 feet away from the wire, and the limit of audibility could not be ascertained. The sound was there quite distinct, although the connections were separated only 10 feet, and did not perceptibly fall off in the last 50 feet, thus pointing to the conclusion that the action is not purely electrostatic. Further trials would be necessary to determine the law of the falling off of the intensity of sound with the increase of the distance from the cable for a given fixed distance between the water connections, as also to determine the law of increase of intensity for any increase of distance between the connections for a given fixed distance from the cable. The experiments in fresh-water shown on fig. 3, made by connecting various wires with a small magneto machine, show the difference in audibility when the distance between the connections was halved.

The next instrument brought before the Society is simply a coil of insulated wire surrounding *a core*, *i.e.* an electro-magnet with a telephone in the circuit of the coil (fig. 2). This instrument was tried at various distances, and the making and breaking of the current produced by 6 dry cells flowing through a wire $\frac{1}{16}''$ diameter and 200 feet in length could be detected 40 feet off, and the current from 12 dry cells can be detected through 60 feet of salt-water. When sunk in water the sound seems just as loud, and there

MR. C. A. STEVENSON ON INDUCTION THROUGH AIR AND WATER.

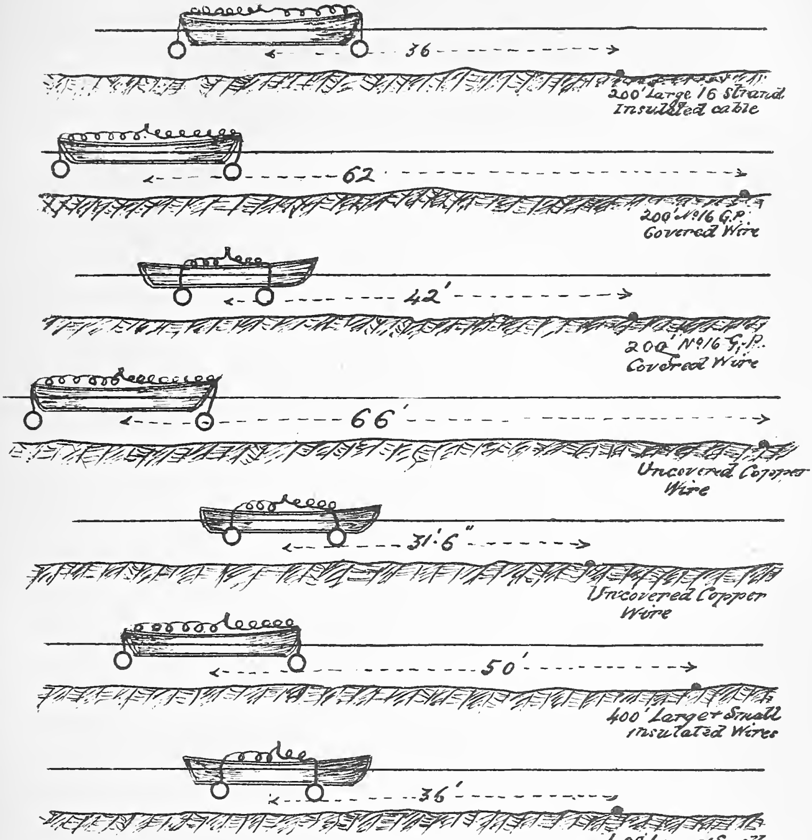



Fig 1


Fig 2 Electromagnetic

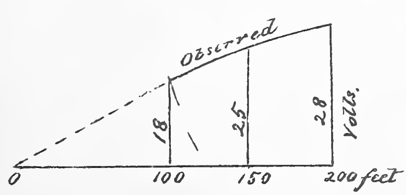



Fig 3.

seems to be no practical difference to the efficiency of the instrument caused by air or by water, salt or fresh.

The action of the instrument appears to be that the break, which is well known as a much more marked phenomenon than the "make," if broken sufficiently rapidly, induces a current in the coil, which the core intensifies immensely, and the effect on the instrument is apparently simply affected by the number of amperes.

I found that the distance at which the instrument was audible in air was proportional to the current in amperes, so that where the external resistance was great in comparison with the resistance of the battery, doubling the wire doubled the current, and so the distance. This is evidently the case in water, as is seen by the observations (fig. 3). The volts were measured by one of Cardrew's voltmeters. It will be readily seen that the distance due to a certain number of amperes is only applicable to the particular instrument in use, and is not the limit, as a more sensitive instrument could be made. The one used was 3 feet in length, with about 2000 turns of wire, varying from $\frac{1}{8}$ to $\frac{1}{32}$ inch in diameter.

The sound in a Bell telephone with this instrument was almost deafening with $3\frac{1}{2}$ fathoms. The result did not appear so effective with a closed circuit of a small magneto machine as with the current produced by dry cells being made and broken.

This electro-magnet system of induction, in contradistinction to the parallel wire or electrostatic systems, has no earth connection, being entirely insulated, and must therefore be a case of true induction through air or water.

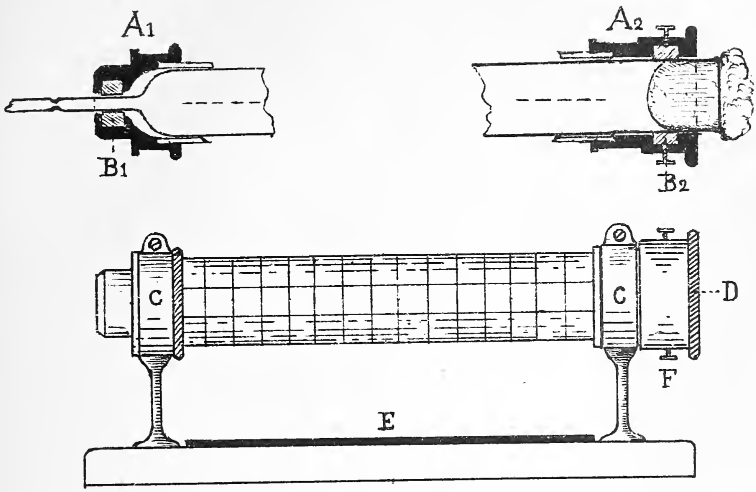
## On a New Apparatus for Counting Bacterial Colonies in Roll-Cultures. By J. Buchanan Young, M.B., B.Sc., Public Health Laboratory, University of Edinburgh. *Communicated by* Sir DOUGLAS MACLAGAN. (With a Plate.)

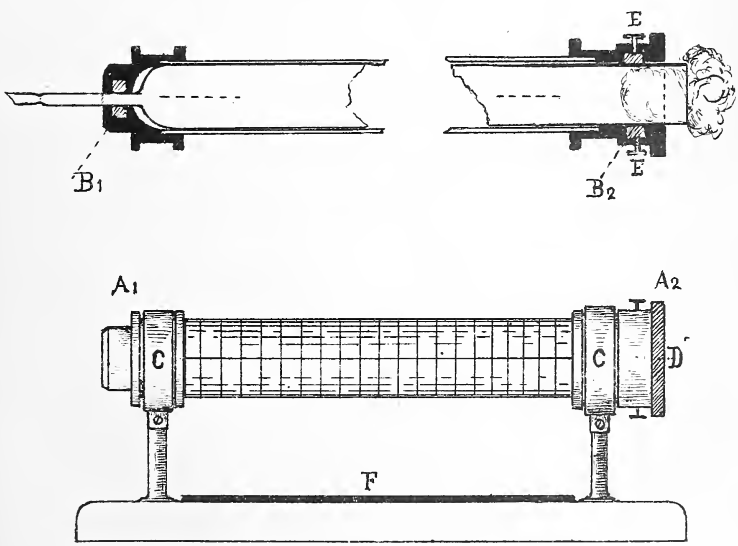
(Read February 6, 1893.)

The apparatus which I have devised for the counting of bacterial colonies in roll-cultures, consists of a glass tube 15 centimetres long, the surface of which is divided by finely etched lines into square centimetres. The bore of this tube is such that the roll-culture tubes, which are of a uniform diameter, just slide without play within it. The ends of the tube are fitted into brass collars (A1, A2), which have inserted in them india-rubber rings (B1, B2). These india-rubber rings serve to retain the culture tubes in position during the process of counting. One end of the apparatus is adapted for grasping ordinary roll-culture tubes, the other for fixing the thin stems of Roux's tubes for anaerobic cultures. The counting tube is capable of being rotated within the split-collars (C,C) by means of the milled collar D, and the culture tube being grasped by one or other india-rubber ring is necessarily rotated along with the counting tube. E is a sheet of optical black glass, which forms a suitable background for showing up minute colonies. F is placed below one of two small pinching screws, which serve to adjust the larger rubber ring for *slight* differences in the diameters of different culture tubes.

To allow of check-counting in case of suspected error, and for the easy localisation of any given colony or colonies, registering-marks are made on the culture tubes by means of the scratching diamond, or other suitable means. These are indicated on the sectional drawings by dotted lines. The mark in the longitudinal axis of the culture tube is, in practice, made to correspond with a specially marked longitudinal line on the counting tube, and that in the transverse axis is brought just to the outer edge of the brass collar A1, or A2, as the case may be. The contents of each longitudinal row of squares are counted seriatim, the counting tube being rotated

MR. J. B. YOUNG ON A NEW APPARATUS FOR COUNTING BACTERIA.



by means of the milled collar for this purpose. The number of colonies in each square is noted on a paper scheme having squares corresponding to those on the counter. When the contents of all the squares have been thus noted, the total is readily obtained.

The apparatus at present in use is that of Von Esmarch, which does not permit of the whole contents of the tube being actually counted. This I consider a distinct disadvantage, as the colonies are never so uniformly distributed in the culture medium, that the counting of the colonies in a given area, or areas, and calculating therefrom, can give more than a rough approximation to the number actually present in any culture tube. It was the recognition of this defect, and a desire to some extent to overcome it, that led me to design the apparatus now described.

The instrument just described is that which I originally designed. I found, however, in practice that special care in counting was necessary when minute colonies came on, or very near to, a longitudinal line. I have therefore found it better to do away with the longitudinal lines, only retaining one of these to act as the registering-mark, and as the starting and ending point in counting.

To use the apparatus as now arranged, we count the number of colonies in the space between two adjacent lines for the whole circumference of the tube, starting at the longitudinal line—rotating the tube as we count—and ending when the line is again reached. Having noted the number of colonies in this space, we now proceed to the next, and so on *seriatim* until the whole tube has been counted.

## Abstract of Paper on the "Digestion of Sugars." By W. G. Aitchison Robertson, M.D., D.Sc., F.R.C.P.E.

(Read February 20, 1893.)

Most individuals consume daily nearly a pound of carbohydrate food in the form of starch or sugar.

The cheapness of sugar in recent times has led to its increased consumption. In 1891 the annual consumpt of cane sugar alone per head of the population was estimated at 80 lbs.

There can be no doubt that the increased consumption of sugar has been followed by an increase in the number of cases of acid dyspepsia, which may in some individuals become so troublesome that they are obliged to abandon saccharine articles of diet as much as possible.

I have been led by the interest and importance of this subject to study some of the changes that cane sugar undergoes during digestion.

When a solution of cane sugar is heated for some time in presence of a dilute mineral acid, it becomes inverted,—that is to say, it links on a molecule of water, and then splits into equal parts of dextrose and lævulose.

Richet affirms, while Hoppe-Seyler denies, that the saliva inverts cane sugar. I have therefore sought to determine this point. I first ascertained the effect of heat alone on a solution of cane sugar. I kept a 20 p.c. solution of cane sugar in distilled water at a temperature of 38° C. for one hour, and found that invert sugar amounting to 0·19 p.c. was produced; and that at the end of two hours it increased to 0·252 p.c.

I then made a 20 p.c. solution of cane sugar in human saliva, and kept it at the same temperature as in the previous case. At the end of one hour the invert sugar amounted to 0·2 p.c., as against 0·19 p.c. in the previous case; while at the end of two hours 0·27 p.c., as against 0·252 p.c. in the preceding experiment.

The amount of invert sugar produced in the two cases was so nearly the same that I think we may safely conclude that the saliva has no effect in hastening the inversion of cane sugar.

I next sought to ascertain the influence of gastric juice on cane sugar. The juice contains from 0·2 to 0·3 p.c. of free hydrochloric acid. To a 20 p.c. solution of cane sugar I added 0·2 p.c. hydrochloric acid, and kept it at 38° C. At the end of one hour it contained 0·37 p.c. invert sugar. After two hours the amount rose to 0·45 p.c.

If we compare the amount of invert sugar formed in the presence of acid with that formed in its absence, we find that the acid nearly doubles the amount produced. Thus—

| Cane Sugar 20 p.c. Sol. at 38° C. | Invert Sugar. | |
|---|---|---|
| | In one hour. | In two hours. |
| (1) Alone . . . | 0·19 p.c. | 0·252 p.c. |
| (2) With 0·2 p.c. HCl . | 0·37 ,, | 0·45 ,, |

At the end of two hours only about $\frac{1}{45}$th part of the cane sugar had undergone inversion in the presence of acid similar in amount to that of the gastric juice, and at the temperature of the stomach.

I then examined the effect of normal gastric juice on cane sugar. With the aid of an œsophageal tube I washed out the stomach of a healthy man, and then injected about three ounces of dilute liquor carnis consisting of the extractives of flesh. One hour later I introduced the stomach-tube and drew off the gastric contents. I filtered this fluid and found that it contained 0·15 p.c. hydrochloric acid, and, as was to be expected, no sugar.

I then made a mixture of equal parts of this healthy gastric fluid, distilled water, and 20 p.c. solution of cane sugar. The sugar in the mixture was thereby reduced to about 6·6 p.c., and the hydrochloric acid to 0·05 p.c. After keeping it at the usual temperature for two hours, the invert sugar formed amounted to 0·83 p.c.

I was so struck by the relatively large amount of invert sugar produced in a fluid containing only a third of the amount of cane sugar and a third of the amount of acid as was contained in the solution I had previously employed, that I made a 6·6 p.c. solution of cane sugar and added to it 0·05 p.c. hydrochloric acid. I kept it at the temperature of the body for two hours, and found that it contained 0·66 p.c. of invert sugar.

The amount of invert sugar formed in this case being only 0·66 p.c., compared with 0·83 p.c. when diluted gastric juice was used, seems to bear out Leube's statement that sugar inversion is less rapid and less energetic with a pure solution of hydrochloric acid than with gastric juice.

It has been definitely stated in some text-books that there is a sugar-inverting ferment in the gastric secretion. I therefore sought to determine whether the inversive action of the acid is aided by such a ferment. I mixed equal volumes of healthy gastric fluid, water, and 20 p.c. solution of cane sugar, and then carefully neutralised the acidity with potassium hydrate. After keeping this mixture at the usual temperature for an hour there was no trace of invert sugar; after two hours a faint trace of sugar had appeared, but too minute for quantitative estimation.

Therefore, when the acidity of gastric juice is neutralised, no inversion of cane sugar occurs. I therefore conclude that there is practically no sugar-inverting ferment in gastric juice, otherwise an appreciable amount of invert sugar would have been formed in the experiment I have detailed.

My experiments have therefore convinced me that the acidity of the gastric juice is the only factor in producing inversion of sugar in the stomach.

I now sought to determine what changes cane sugar undergoes when introduced into the stomach. I experimented on a healthy man by the following method:—At 8 A.M. he had breakfast of porridge and milk. Two hours afterwards I washed out his stomach with a syphon tube, and then poured 250 c.c. of a warm 20 p.c. solution of pure cane sugar into the stomach. At intervals of from half an hour to two hours after the injection I passed the stomach-tube and removed some of the gastric contents. Each sample of the gastric fluid so obtained was measured and filtered, and the reducing sugar in it estimated by Fehling's solution. The total acidity was estimated with a centinormal solution of caustic soda, and expressed in terms of anhydrous hydrochloric acid.

Having done this, I inverted the unchanged cane sugar in a definite amount of the gastric fluid by heating it for two hours with dilute sulphuric acid, by which the whole sugar was inverted.

My experiments show—

(*a*) That inversion of cane sugar really takes place in the healthy stomach.

(*b*) That as digestion proceeds, the proportion of invert sugar increases.

(*c*) That the amount of invert sugar formed is proportional to the acidity of the gastric juice.

I therefore conclude that the inversion of cane sugar which occurs in the stomach is relatively small in amount, and is due to the acid of the gastric juice, and not to a special inverting ferment. The great inversion of cane sugar takes place in the small intestine, and is there induced by a sugar-inverting ferment in the intestinal juice, as shown by previous observers, whose results can be easily confirmed.

I thought it would be interesting to ascertain what happens to invert sugar in the stomach. I accordingly inverted a 20 p.c. solution of cane sugar by heating it for several hours with a small amount of dilute sulphuric acid. I injected 250 c.c. of this solution into the man's stomach, after washing it out in the manner already described. At suitable intervals afterwards I removed some of the gastric contents and estimated the amount of sugar. I found that invert sugar leaves the stomach much more rapidly than cane sugar.

To try the practical importance of these results, I conducted a series of experiments on the digestion of cane and invert sugar in diseased conditions of the body. Amongst the patients whom I experimented on were those suffering from chronic gastric catarrh, pernicious anæmia, alcoholic gastritis, &c. In almost every case these individuals made complaint of weight, pain, heartburn, flatulence, &c., after the solution of cane sugar had been injected, while there was an entire absence of disagreeable sensations after the same amount of invert sugar had been injected into the stomach.

In cases of chronic gastric catarrh, cane sugar is retained in the stomach for a lengthened period, but does not undergo any marked degree of inversion. This is contrary to Schiff's statement that in cases where there is a great secretion of mucus, cane sugar is readily converted into glucose and lævulose. When invert sugar is given, however, to such a patient, it very rapidly disappears from the stomach, though not so quickly as in the case of normal digestion.

In a case of pernicious anæmia the gastric juice had practically no acidity, and consequently cane sugar introduced into the stomach underwent no inversion, even after an interval of two hours. Cane sugar also caused great pain and discomfort when introduced into the patient's stomach. A solution of invert sugar of the same strength injected into his empty stomach gave rise to no symptoms whatever, but was very rapidly passed out of the stomach.

From these experiments I am convinced that cane sugar lingers in the stomach for a much longer period than invert sugar, and is consequently much more liable to undergo fermentative changes. In nearly all cases of indigestion cane sugar is badly borne, while invert sugar is much more easily tolerated. This appears to me to be a point of considerable practical importance, for it is easy to give invert sugar in place of cane sugar in persons whose digestion proceeds slowly.

## On Properties of the Parabola. By Professor Anglin.

(Read February 6, 1893.)

In the following pages we propose to obtain expressions for the several parts of a parabola (axis, focus, vertex, directrix, tangent at vertex, and parameter), whose equation is expressed in the most general form in Cartesian co-ordinates.

Some of the results obtained have already appeared, in which cases we shall supply alternative proofs. We shall also endeavour to complete the investigation of the whole subject.

I. When the curve is referred to rectangular axes.

1. To find the *axis*, *tangent at vertex*, and *parameter*. These are obtained in Smith's *Conic Sections*, § 172, in a very neat manner; but we may replace this method with a little variation at the beginning.

(1.) Taking the usual equation

$$(\alpha x+\beta y)^2+2gx+2fy+c=0\,,$$

we see that $\alpha x+\beta y=0$ is a diameter since it meets the curve in only one point, namely, where it meets the line $2gx+2fy+c=0$; and this is the tangent at its extremity since it meets the curve in the two coincident points given by its equation and $(\alpha x+\beta y)^2=0$. Thus the axis is $\alpha x+\beta y+\lambda=0$, where $\lambda$ is a constant; and since the equation to the curve is

$$(\textit{diameter})^2+\textit{tangent at extremity}=0\,,$$

we obtain $\lambda$ by the condition that these lines are at right angles, getting

$$\lambda=(\alpha g+\beta f)/(\alpha^2+\beta^2)\,.$$

Also, since

$$\frac{\lambda\alpha-g}{\beta}=\frac{\lambda\beta-f}{-\alpha}=\frac{\alpha f-\beta g}{\alpha^2+\beta^2}\,,$$

the tangent at vertex is

$$2(\alpha f-\beta g)(\beta x-\alpha y)+(\alpha^2+\beta^2)(\lambda^2-c)=0\,;$$

and the parameter is

$$2(\alpha f-\beta g)/(\alpha^2+\beta^2)^{\frac{3}{2}}.$$

(2.) We may also proceed as follows :—

Since the axis is $\alpha x+\beta y+\lambda=0$, the tangent at vertex will be $\beta x-\alpha y+\mu=0$, and the equation to the parabola consequently

$$(\alpha x+\beta y+\lambda)^2=2\kappa(\beta x-\alpha y+\mu),$$

in which the parameter is obviously $2\kappa/\sqrt{\alpha^2+\beta^2}$.

We find $\lambda$, $\mu$, $\kappa$ by comparing this equation with the given one. Writing it in the form

$$(\alpha x+\beta y)^2+2(\alpha\lambda-\beta\kappa)x+2(\beta\lambda+\alpha\kappa)y+\lambda^2-2\kappa\mu=0$$

we have

$$1=\frac{\alpha\lambda-\beta\kappa}{g}=\frac{\beta\lambda+\alpha\kappa}{f}=\frac{\lambda^2-2\kappa\mu}{c},$$

and therefore

$$1=\frac{(\alpha^2+\beta^2)\lambda}{\alpha g+\beta f}=\frac{(\alpha^2+\beta^2)\kappa}{\alpha f-\beta g},$$

giving $\lambda$ and $\kappa$. Thus the parameter is

$$2(\alpha f-\beta g)/(\alpha^2+\beta^2)^{\frac{3}{2}};$$

and since $2\kappa\mu=\lambda^2-c$, the tangent at vertex is

$$2(\alpha f-\beta g)(\beta x-\alpha y)+(\alpha^2+\beta^2)(\lambda^2-c)=0.$$

(3.) The axis may also be found immediately, by simply expressing the condition that the diameter $\alpha x+\beta y=0$ shall be at right angles to the tangent at any point

$$(\alpha x+\beta y)(\alpha x'+\beta y')+g(x+x')+f(y+y')+c=0,$$

when we get

$$\alpha^2(\alpha x'+\beta y')+\alpha g+\beta^2(\alpha x'+\beta y')+\beta f=0,$$

that is,

$$(\alpha^2+\beta^2)(\alpha x+\beta y)+\alpha g+\beta f=0,$$

which is the equation to the axis.

2. To find the co-ordinates of the *vertex*.

These may be found in several ways. By regarding the vertex as intersection of the axis and tangent at vertex, or rather more simply, as the intersection of the axis and curve, we have to solve the simultaneous equations

$$\left.\begin{array}{l}\alpha x+\beta y\;+\lambda=0\\ 2gx+2fy=\lambda^2+c=0\end{array}\right\},$$

when we have at once

$$2(af-\beta g)x=\lambda^2\beta-2\lambda f+\beta c$$

and

$$2(af-\beta g)y=\lambda^2\alpha-2\lambda g+\alpha c\,.$$

Substituting for $\lambda$ its value, and reducing, we shall get

$$\begin{aligned}2(\alpha f-\beta g)(\alpha^2+\beta^2)^2\,.\,x\\ &=\beta c(\alpha^2+\beta^2)^2+(\alpha g+\beta f)(\alpha\beta g-\beta^2 f-2\alpha^2 f)\\ 2(\alpha f-\beta g)(\alpha^2+\beta^2)^2\,.\,y\\ &=\alpha c(\alpha^2+\beta^2)^2+(\alpha g+\beta f)(\alpha\beta f-\alpha^2 g-2\beta^2 g)\,.\end{aligned}$$

3. To find the *focus* and *directrix.*

(1.) By the focus and directrix property. Let the focus be $(h,k)$ and the directrix $x\cos\theta+g\sin\theta-p=0$, so that the equation to the curve is

$$(x-h)^2+(y-k)^2=(x\cos\theta+y\sin\theta-p)^2,$$

which may be written

$$(x\sin\theta-y\cos\theta)^2+2x(p\cos\theta-h)+2y(p\sin\theta-k)\\ +h^2+k^2-p^2=0\,.$$

Making this identical with the given equation, we have

$$\frac{\sin^2\theta}{\alpha^2}=\frac{\cos^2\theta}{\beta^2}=\frac{p\cos\theta-h}{g}=\frac{p\sin\theta-k}{f}=\frac{h^2+k^2-p^2}{c}\,.$$

Thus
$$h=p\cos\theta-g/(\alpha^2+\beta^2)$$

and
$$k=p\sin\theta-f/(\alpha^2+\beta^2)\,,$$

$$\therefore\quad h^2+k^2-p^2=(f^2+g^2)/(\alpha^2+\beta^2)^2-2p(f\sin\theta+g\cos\theta)/(\alpha^2+\beta^2)\,,$$

and
$$2p(f\sin\theta+g\cos\theta)(\alpha^2+\beta^2)=f^2+g^2-c(\alpha^2+\beta^2)\,.$$

Now $\theta$, being the inclination of the axis to the initial line, is obtuse; and thus we have

$$2p(\alpha f-\beta g)\sqrt{\alpha^2+\beta^2}=f^2+g^2-c(\alpha^2+\beta^2)\,.$$

But the directrix is

$$\beta x-\alpha y+p\sqrt{\alpha^2+\beta^2}=0\,,$$

and hence it is

$$2(\alpha f-\beta g)(\beta x-\alpha y)+f^2+g^2-c(\alpha^2+\beta^2)=0\,.$$

Also,
$$-(\alpha^2+\beta^2)h\ =\ g+\beta p\sqrt{\alpha^2+\beta^2}$$

and $$(\alpha^2+\beta^2)k = -f + \alpha p\sqrt{\alpha^2+\beta^2}\,;$$

hence

$$2(\alpha f-\beta g)(\alpha^2+\beta^2)h = \beta(g^2-f^2)+\beta c(\alpha^2+\beta^2)-2\alpha fg\,,$$
$$2(\alpha f-\beta g)(\alpha^2+\beta^2)k = \alpha(g^2-f^2)-\alpha c(\alpha^2+\beta^2)+2\beta fg\,.$$

(2.) We may also find the directrix as the locus of the intersection of tangents at right angles.

If $\phi(x,y)=0$ represent the curve, the equation to the pair of tangents from $(x',y')$ is

$$\phi(x,y)\phi(x',y') = \{(\alpha x+\beta y)(\alpha x'+\beta y')+g(x+x')+f(y+y')+c\}^2,$$

which will be at right angles if

$$(\alpha^2+\beta^2)\phi(x',y') = \{\alpha(\alpha x'+\beta y')+g\}^2+\{\beta(\alpha x'+\beta y')+f\}^2,$$

and thus the locus of their intersection is

$$(\alpha^2+\beta^2)(2gx+2fy+c) = 2(\alpha x+\beta y)(\alpha g+\beta f)+f^2+g^2\,,$$

that is,

$$2(\alpha f-\beta g)(\beta x-\alpha y)+f^2+g^2-c(\alpha^2+\beta^2)=0\,,$$

which is the equation to the directrix.

(3.) The focus may also be found as the pole of the directrix.

Making the polar of $(x',y')$, namely,

$$(\alpha x+\beta g)(\alpha x'+\beta y')+g(x+x')+f(y+y')+c=0\,,$$

identical with the equation to the directrix, we have

$$\frac{\alpha(\alpha x'+\beta y')+g}{2\beta} = \frac{\beta(\alpha x'+\beta y')+f}{-2\alpha} = \frac{(\alpha f-\beta g)(gx'+fy'+c)}{f^2+g^2-c(\alpha^2+\beta^2)}\,,$$

each of which ratios $=(\beta g-\alpha f)/2(\alpha^2+\beta^2)$.

Thus $x'$ and $y'$ are given by

$$\left.\begin{array}{r}(\alpha^2+\beta^2)(\alpha x+\beta y)+\alpha g+\beta f=0\\ 2(\alpha^2+\beta^2)(gx+fy)+f^2+g^2+c(\alpha^2+\beta^2)=0\end{array}\right\}\,;$$

and hence

$$2(\alpha f-\beta g)(\alpha^2+\beta^2)x = \beta(g^2-f^2)+\beta c(\alpha^2+\beta^2)-2\alpha fg$$
$$2(\alpha f-\beta g)(\alpha^2+\beta^2)y = \alpha(g^2-f^2)-\alpha c(\alpha^2+\beta^2)+2\beta fg\,.$$

II. When the curve is referred to *oblique* axes.

The general equation to a parabola is of the same form in the oblique as in the rectangular system.

1. The axis, tangent at vertex, and parameter may be found in a manner exactly analogous to that for rectangular axes.

(1.) The axis being $\alpha x+\beta y+\lambda=0$, and writing the equation to the curve

$$(\alpha x+\beta y+\lambda)^2+2(g-\lambda\alpha)x+2(f-\lambda\beta)y+c-\lambda^2=0\,,$$

the lines

$$\alpha x+\beta y+\lambda=0\,,\ \text{and}\ 2(\lambda\alpha-g)x+2(\lambda\beta-f)y+\lambda^2-c=0$$

will be at right angles, if

$$\alpha(\lambda\alpha-g)+\beta(\lambda\beta-f)-\{\alpha(\lambda\beta-f)+\beta(\lambda\alpha-g)\}\cos\omega=0,$$

or if,

$$(\alpha^2+\beta^2-2\alpha\beta\cos\omega)\lambda=\alpha g+\beta f-(\alpha f+\beta g)\cos\omega\,.$$

Thus, denoting $\alpha^2+\beta^2-2\alpha\beta\cos\omega$ by $\gamma^2$, the axis is

$$(\alpha x+\beta y)\gamma^2+\alpha g+\beta f-(\alpha f+\beta g)\cos\omega=0\,;$$

and, since

$$\frac{\lambda\alpha-g}{\beta-\alpha\cos\omega}=-\frac{\lambda\beta-f}{\alpha-\beta\cos\omega}=\frac{\alpha f-\beta g}{\gamma^2}\,,$$

the tangent at vertex is

$$2(\alpha f-\beta g)\{(\beta-\alpha\cos\omega)x-(\alpha-\beta\cos\omega)y\}+(\lambda^2-c)\gamma^2=0\,.$$

To find the parameter, we write equation to curve in form

$$(\textit{perp. on axis})^2=p(\textit{perp. on tang. at vertex})\,,$$

where $p$ is the parameter; that is,

$$\frac{(\alpha x+\beta y+\lambda)^2\sin^2\omega}{\alpha^2+\beta^2-2\alpha\beta\cos\omega}=p\cdot\frac{\{2(\lambda\alpha-g)x+2(\lambda\beta-f)y+\lambda^2-c\}\sin\omega}{\sqrt{\{4(\lambda\alpha-g)^2+4(\lambda\beta-f)^2-8(\lambda\alpha-g)(\lambda\beta-f)\cos\omega\}}}\,;$$

hence

$$p\gamma^2=2\sqrt{\{(\lambda\alpha-g)^2+(\lambda\beta-f)^2-2(\lambda\alpha-g)(\lambda\beta-f)\cos\omega\}}\sin\omega\,.$$

Substituting for $\lambda\alpha-g$ and $\lambda\beta-f$ their values, and observing that

$$(\beta-\alpha\cos\omega)^2+(\alpha-\beta\cos\omega)^2+2(\beta-\alpha\cos\omega)(\alpha-\beta\cos\omega)\cos\omega$$
$$=\gamma^2\sin^2\omega\,,$$

we get

$$p\gamma^2=2(\alpha f-\beta g)\sin^2\omega/\gamma\,;$$

and thus

$$p=2(\alpha f-\beta g)\sin^2\omega/\gamma^3.$$

(2.) Proceeding similarly to the second method in the rectangular system, the axis being $\alpha x+\beta y+\lambda=0$, the tangent at vertex will be

$$(\beta-\alpha\cos\omega)x-(\alpha-\beta\cos\omega)y+\mu=0\,,$$

since it is perpendicular to the axis, and the equation to the parabola consequently

$$(\alpha x+\beta y+\lambda)^2=2\kappa\{(\beta-\alpha\cos\omega)x-(\alpha-\beta\cos\omega)y+\mu\},$$

in which the parameter is $2\kappa\sin^2\omega/\gamma$, which may be seen by writing equation to curve in the form already indicated.

We find $\lambda$, $\mu$, $\kappa$ by writing equation to curve in form

$$(\alpha x+\beta y)^2+2\{\alpha\lambda-(\beta-\alpha\cos\omega)\kappa\}x+2\{\beta\lambda+(\alpha-\beta\cos\omega)\kappa\}y$$
$$+\lambda^2-2\kappa\mu=0\,,$$

and comparing with the given equation, when we have

$$1=\frac{\alpha\lambda-(\beta-\alpha\cos\omega)\kappa}{g}=\frac{\beta\lambda+(\alpha-\beta\cos\omega)\kappa}{f}=\frac{\lambda^2-2\kappa\mu}{c}\,,$$

and therefore

$$1=\frac{\lambda\gamma^2}{\alpha g+\beta f-(\alpha f+\beta g)\cos\omega}=\frac{\kappa\gamma^2}{\alpha f-\beta g}\,,$$

giving $\lambda$ and $\kappa$. Thus the parameter is

$$2(\alpha f-\beta g)\sin^2\omega/\gamma^3\,;$$

and since $2\kappa\mu=\lambda^2-c$, the tangent at vertex is

$$(\beta-\alpha\cos\omega)x-(\alpha-\beta\cos\omega)y+(\lambda^2-c)\gamma^2/2(\alpha f-\beta g)=0\,.$$

(3.) The axis may also be found by simply expressing the condition that the diameter $\alpha x+\beta y=0$ shall be at right angles to the tangent at any point, when we get

$$\alpha^2(\alpha x'+\beta y')+\alpha g+\beta^2(\alpha x'+\beta y')+\beta f$$
$$-\{\alpha\beta(\alpha x'+\beta y')+\alpha f+\alpha\beta(\alpha x'+\beta y')+\beta g\}\cos\omega=0$$

that is

$$(\alpha x+\beta y)\gamma^2+\alpha g+\beta f-(\alpha f+\beta g)\cos\omega=0\,,$$

which is the equation to the axis.

2. To find the co-ordinates of the *vertex*.

As in the case of rectangular axes, we have

$$2(\alpha f-\beta g)x=\lambda(\lambda\beta-2f)+c\beta$$

and

$$2(\alpha f-\beta g)y=\lambda(\lambda\alpha-2g)+c\alpha\,.$$

Substituting for $\lambda$ its value in the expressions $\lambda\beta - 2f$ and $\lambda\alpha - 2g$, and reducing, we shall get

$$2(\alpha f - \beta g)\gamma^2.x$$
$$= c\beta\gamma^2 + \lambda\{\alpha\beta g - \beta^2 f - 2\alpha^2 f + (3\alpha\beta f - \beta^2 g)\cos\omega\}$$
$$2(\alpha f - \beta g)\gamma^2.y$$
$$= c\alpha\gamma^2 + \lambda\{\alpha\beta f - \alpha^2 g - 2\beta^2 g + (3\alpha\beta g - \alpha^2 f)\cos\omega\}.$$

3. To find the *focus* and *directrix.*

(1.) By the focus and directrix property. Taking, for the present, the equation to the parabola as

$$(ax - by)^2 + 2gx + 2fy + c = 0\,,$$

let the focus be $(h, k)$ and the directrix $x\cos\alpha + y\cos\beta - p = 0$, so that the equation to the curve is

$$(x-h)^2 + (y-k)^2 + 2(x-h)(y-k)\cos\omega = (x\cos\alpha + y\cos\beta - p)^2,$$

which may be written

$$(x\sin\alpha - y\sin\beta)^2 + 2x(p\cos\alpha - k\cos\omega - h)$$
$$+ 2y(p\cos\beta - h\cos\omega - k) + h^2 + k^2 + 2hk\cos\omega - p^2 = 0\,.$$

Making this identical with the given equation, we have

$$\frac{\sin^2\alpha}{a^2} = \frac{\sin^2\beta}{b^2} = \frac{p\cos\alpha - k\cos\omega - h}{g} = \frac{p\cos\beta - h\cos\omega - k}{f}$$
$$= (h^2 + k^2 + 2hk\cos\omega - p^2)/c\,.$$

Now, since the line $ax - by = 0$ makes angles $\alpha$ and $\beta$ with the axes, we have

$$\cot\alpha = (b + a\cos\omega)/a\sin\omega\,, \quad \cot\beta = (a + b\cos\omega)/b\sin\omega\,;$$

from which

$$\sin\alpha = a\sin\omega/\gamma\,, \quad \cos\alpha = (b + a\cos\omega)/\gamma\,,$$
$$\sin\beta = b\sin\omega/\gamma\,, \quad \cos\beta = (a + b\cos\omega)/\gamma\,,$$

where $\gamma^2 = a^2 + b^2 + 2ab\cos\omega$.

Thus

$$h + k\cos\omega = p(b + a\cos\omega)/\gamma - g\sin^2\omega/\gamma^2$$
$$k + h\cos\omega = p(a + b\cos\omega)/\gamma - f\sin^2\omega/\gamma^2\,;$$

from which

$$\gamma^2 h = b\gamma p + f\cos\omega - g$$
$$\gamma^2 k = a\gamma p + g\cos\omega - f\,.$$

Hence, observing that

$$(f\cos\omega - g)^2 + (g\cos\omega - f)^2 + 2(f\cos\omega - g)(g\cos\omega - f)\cos\omega$$
$$= (f^2 + g^2 - 2fg\cos\omega)\sin^2\omega\,,$$

we have

$$(h^2 + k^2 + 2hk\cos\omega - p^2)\gamma^4$$
$$= (f^2 + g^2 - 2fg\cos\omega)\sin^2\omega - 2p(af + bg)\gamma\sin^2\omega\,;$$

and thus

$$2(af + bg)\gamma p = f^2 + g^2 - 2fg\cos\omega - c\gamma^2.$$

But the directrix is

$$(b + a\cos\omega)x + (a + b\cos\omega)y - \gamma p = 0\,,$$

and hence, replacing $a$ and $b$ by $\alpha$ and $-\beta$, its equation is

$$2(\alpha f - \beta g)\{(\beta - \alpha\cos\omega)x - (\alpha - \beta\cos\omega)y\}$$
$$+ f^2 + g^2 - 2fg\cos\omega - c\gamma^2 = 0\,.$$

Also, substituting for $\gamma p$ its value in the equations for the focus, we have on reduction

$$2(\alpha f - \beta g)\gamma^2.h$$
$$= \beta(g^2 - f^2) + c\beta\gamma^2 + 2\alpha f(f\cos\omega - g)\,,$$
$$2(\alpha f - \beta g)\gamma^2.k$$
$$= \alpha(g^2 - f^2) - c\alpha\gamma^2 - 2\beta g(g\cos\omega - f)\,.$$

(2.) The pair of tangents from $(x',y')$ whose equation is

$$\phi(x,y)\phi(x',y') = \{(\alpha x + \beta y)(\alpha x' + \beta y') + g(x + x') + f(y + y') + c\}^2$$

will be at right angles if

$$(\alpha^2 + \beta^2 - 2\alpha\beta\cos\omega)\phi(x',y')$$
$$= \{\alpha(\alpha x' + \beta y') + g\}^2 + \{\beta(\alpha x' + \beta y') + f\}^2 - 2\{\alpha(\alpha x' + \beta y') + g\}\{\beta(\alpha x' + \beta y') + f\}\cos\omega\,;$$

and thus the locus of their intersection is

$$(\alpha^2 + \beta^2 - 2\alpha\beta\cos\omega)(2gx + 2fy + c)$$
$$= 2(\alpha x + \beta y)\{\alpha g + \beta f - (\alpha f + \beta g)\cos\omega\} + f^2 + g^2 - 2fg\cos\omega\,,$$

that is,

$$2(\alpha f - \beta g)\{(\beta - \alpha\cos\omega)x - (\alpha - \beta\cos\omega)y\}$$
$$+ f^2 + g^2 - 2fg\cos\omega - c\gamma^2 = 0\,,$$

which is the equation to the directrix.

(3.) Making the polar of $(x',y')$ identical with the equation to the directrix, we have

$$\frac{\alpha(\alpha x'+\beta y')+g}{2(\beta-\alpha\cos\omega)}=\frac{\beta(\alpha x'+\beta y')+f}{-2(\alpha-\beta\cos\omega)}$$
$$=\frac{(\alpha f-\beta g)(gx'+fy'+c)}{f^2+g^2-2fg\cos\omega-c\gamma^2},$$

each of which ratios $=(\beta g-\alpha f)/2\gamma^2$.

Thus $x'$ and $y'$, the co-ordinates of the focus, are given by

$$\left.\begin{array}{l}(\alpha x+\beta y)\gamma^2+\alpha g+\beta f-(\alpha f+\beta g)\cos\omega=0\\ 2(gx+fy)\gamma^2+f^2+g^2-2fg\cos\omega+c\gamma^2=0\end{array}\right\};$$

and hence

$$2(\alpha f-\beta g)\gamma^2.x$$
$$=\beta(g^2-f^2)+c\beta\gamma^2+2\alpha f(f\cos\omega-g),$$
$$2(\alpha f-\beta g)\gamma^2.y$$
$$=\alpha(g^2-f^2)-c\alpha\gamma^2-2\beta g(g\cos\omega-f).$$

III. When the curve is referred to a pair of tangents as axes.

If $a$, $b$ be the lengths of the axes, the equation to the curve may be written

$$\left(\frac{x}{a}-\frac{y}{b}\right)^2-\frac{2x}{a}-\frac{2y}{b}+1=0,$$

so that all the results may either be at once deduced from those previously obtained for oblique axes, or they may be found independently in like manner. It is, however, desirable to investigate the results in this case by peculiar methods, on account of certain tangent properties of the curve.

Taking the usual figure, complete the parallelogram QOQ′W. Then the triangles OSQ′, OQW are equiangular,

$$\therefore\quad OS.OW=OQ.OQ'.$$

Also, drawing SR parallel to OQ, the triangle ORS is equiangular to each of the triangles OSQ, OSQ′;

$$\therefore\quad OR.OQ'=OS^2=SR.OQ.$$

Further, since $\angle MOM'=2\angle QOQ'$, the $\angle MOV=\angle QOQ'$.

Hence, if OQ′, OQ be axes of $x$ and $y$ respectively, and $\angle QOQ'=\omega$, we have

$$ax=by=OS^2=a^2b^2/c^2,$$

where

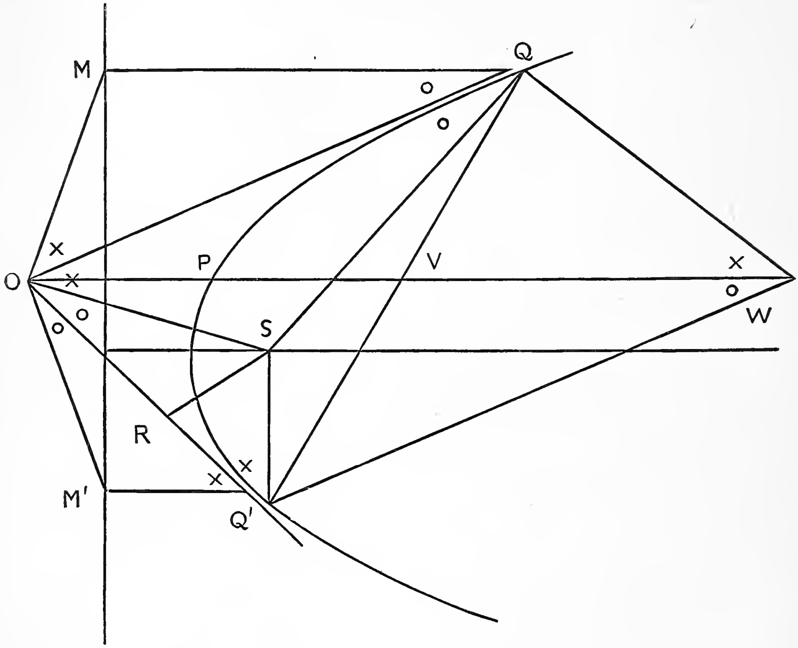
$$c^2 = OW^2 = a^2 + b^2 + 2ab \cos \omega \,;$$

and thus the *focus.*

Since $bx - ay = 0$ is a diameter, the directrix is

$$x(a + b \cos \omega) + y(b + a \cos \omega) - \mu = 0 \,,$$

being perpendicular to a diameter; and $\mu$ is found at once, since




the perpendicular from O on MM′ $=$ OS cos $\omega$. Thus, observing that

$$(a + b \cos \omega)^2 + (b + a \cos \omega)^2 - 2(a + b \cos \omega)(b + a \cos \omega) \cos \omega = c^2 \sin^2 \omega,$$

we have

$$\frac{\mu}{c} = OS \cos \omega = \frac{ab \cos \omega}{c}, \quad \text{and } \mu = ab \cos \omega \,,$$

which gives the *directrix.*

The axis is $bx - ay - \lambda = 0$; and $\lambda$ is most easily found by the condition that the focus is on the line. Thus

$$\lambda = ab(b^2 - a^2)/c^2,$$

and hence the *axis.*

The *parameter* may be found conveniently in two ways.

(1.) The perpendicular from the focus on the directrix

$$=ab\{2ab+(a^2+b^2-c^2)\cos\omega\}/c^3=2a^2b^2\sin^2\omega/c^3\,;$$

and thus the parameter $=4a^2b^2\sin^2\omega/c^3$.

(2.) Since $\qquad QV^2=4SP\,.\,PV\,,$

we have $\qquad p\,.\,PV=QV^2\sin^2\theta\,,$

where $p$ is the parameter, and $\theta=\angle OVQ'$.

But

$$ab\sin\omega=2\,\Delta\,OQQ'=2OV\,.\,QV\sin\theta\,;$$

$$\therefore\qquad a^2b^2\sin^2\omega=4p\,.\,OV^2.\,PV\,;$$

$$\therefore\qquad p=4a^2b^2\sin^2\omega/c^3.$$

Two methods for finding the *tangent at the vertex* may also be shown.

(1.) Its equation is

$$x(a+b\cos\omega)+y(b+a\cos\omega)-\mu=0\,\cdot$$

The perpendicular on this from the focus being equal to one-fourth of the parameter, we have

$$\{2a^2b^2+ab(a^2+b^2)\cos\omega-\mu c^2\}/c^3=a^2b^2\sin^2\omega/c^3\,;$$

$$\therefore\qquad \mu c^2/ab=(a^2+b^2)\cos\omega+ab(1+\cos^2\omega)$$

$$=(a+b\cos\omega)(b+a\cos\omega)\,;$$

and thus the equation is

$$x/(b+a\cos\omega)+y/(a+b\cos\omega)=ab/c^2.$$

(2.) By expressing the condition that the line shall touch the curve.

The equation to the tangent at $(x', y')$ being

$$x/\sqrt{ax'}+y/\sqrt{by'}=1\,,$$

will be identical with the above equation, if

$$\frac{\mu}{a+b\cos\omega}=\sqrt{ax'},\qquad \frac{\mu}{b+a\cos\omega}=\sqrt{by'}\,.$$

Thus

$$\frac{1}{\mu}=\frac{1}{a(a+b\cos\omega)}+\frac{1}{b(b+a\cos\omega)}=c^2/ab(a+b\cos\omega)(b+a\cos\omega)\,.$$

This method also gives the co-ordinates of the *vertex,* when we have

$$\sqrt{ax} = ab(b + a \cos \omega)/c^2$$

$$\sqrt{by} = ab(a + b \cos \omega)/c^2 .$$

We may also find the vertex as the intersection of the axis and curve, when we get

$$bx - ay = \lambda$$

$$2ab(bx + ay) = \lambda^2 + a^2b^2.$$

Thus

$$4ab^2x = (ab + \lambda)^2 = 4a^2b^4(b + a \cos \omega)^2/c^4,$$

$$\therefore \qquad x = ab^2(b + a \cos \omega)^2/c^4 ;$$

and

$$4a^2by = (ab - \lambda)^2 = 4a^4b^2(a + b \cos \omega)^2/c^4,$$

$$\therefore \qquad y = a^2b(a + b \cos \omega)^2/c^4.$$

## Preliminary Note on Observations of the Minor Planet Victoria in 1889. By Dr D. Gill, H.M. Astronomer at the Cape of Good Hope.

(Read March 20, 1893.)

The object of the work was—

1st, To determine the mean solar horizontal parallax.

2nd, To compare the tabular and observed motion of the planet, with a view to ascertain whether any periodic perturbations of short period occur,—such, for example, as would be accounted for by an error in the adopted value of the lunar equation.

The results depend on the following series of observations :—

A. Meridian observations of the comparison stars and of the planet made at twenty-one different observatories during the opposition of 1889.

B. The heliometric triangulation of the comparison stars ; that is to say, measurement of the mutual distances of all the stars which are within 2 degrees of arc of each other (the range of measurement of modern heliometers), supplemented by measures of the position angles, when such measures were necessary in consequence of unfavourable geometric conditions of rigidity from observations of distance only. These observations were executed during the whole period when the stars were visible in 1890 at Yale, Göttingen, Bamberg, and the Cape.

C. Heliometer observations of the angular distance of the planet from two opposite comparison stars, one vertically above and one below the planet in the evening, and from another pair of stars, one vertically above and one below the planet in the early morning. The work was carried out at Yale, Leipzig, Göttingen, and Bamberg, in the northern hemisphere, and at the Cape in the southern hemisphere, in strict accordance with a previously prepared programme.

Eight hundred of such observations were secured in each hemisphere ; and during the period June 15 to August 27 there are only six nights on which observations were not secured at one or more of the observatories. Professor A. Auwers of Berlin rendered an inestimable service to science by coming to the Cape and sharing with me the work of observing the planet in 1889.

It is impossible, within limits of a communication like the present, to do further than state the general results arrived at.

A discussion of the combined meridian and heliometer observations in the triangulation gives positions of the comparison stars, of which the probable error in either co-ordinate hardly exceeded $\pm 0''\cdot03$ (see *Astron. Nach.*, Nos. 3107–8).

The observations were discussed in groups, with the following results for the mean solar horizontal parallax :—

| | | | Limits of Group. | | Mean Solar Parallax. | | Relative Weight. |
|---|---|---|---|---|---|---|---|
| Group | I. | June | 10–12 | ... | 8″·723 | ... | 0·8 |
| ,, | II. | ,, | 15–19 | ... | ·804 | ... | 12·3 |
| ,, | III. | ,, | 19–26 | ... | ·828 | ... | 15·4 |
| ,, | IV. | ,, | 26–July 3 | ... | ·872 | ... | 29·2 |
| ,, | V. | July | 3–9 | ... | ·789 | ... | 9·8 |
| ,, | VI. | ,, | 9–12 | ... | ·857 | ... | 17·5 |
| ,, | VII. | ,, | 15–20 | ... | ·793 | ... | 19·5 |
| ,, | VIII. | ,, | 20–23 | ... | ·809 | ... | 20·0 |
| ,, | IX. | ,, | 23–25 | ... | ·742 | ... | 14·0 |
| ,, | X. | ,, | 25–28 | ... | ·806 | ... | 11·2 |
| ,, | XI. | ,, | 28–Aug. 4 | ... | ·777 | ... | 33·4 |
| ,, | XII. | Aug. | 4–10 | ... | ·826 | ... | 20·0 |
| ,, | XIII. | ,, | 10–17 | ... | ·816 | ... | 26·0 |
| ,, | XIV. | ,, | 17–22 | ... | ·819 | ... | 19·9 |
| ,, | XV. | ,, | 22–27 | ... | ·738 | ... | 13·3 |

Whence in the mean

$$\pi = 8''\cdot809, \text{ prob. error } \pm 0''\cdot0066.$$

From the diurnal parallax derived from Cape observations only, there resulted—

$$\pi = 8''\cdot822, \text{ prob. error } \pm 0''\cdot014.$$

It was further found that the observed and tabular places of the planet could not be represented within possible error of observation without a periodic term nearly identical with that of the moon's period, having its maximum and minimum corresponding with the epochs when the moon's longitude differed 90° from that of the planet. But if Leverrier's value of the lunar equation (6″·50), which was employed in the computation of the ephemeris, be reduced to (6″·40), the corrected ephemeris gives the following very small residuals when compared with the observations :—

| Group. | $\Delta\alpha$. C – O. | | $\Delta\delta$. C – O. |
|---|---|---|---|
| II. | – 0″·01 | ... | + 0″·00 |
| III. | + ·04 | ... | + ·02 |
| IV. | – ·02 | ... | – ·07 |
| V. | – ·13 | ... | + ·03 |
| VI. | + ·09 | ... | + ·04 |
| VII. | + ·07 | ... | + ·12 |
| VIII. | – ·05 | ... | + ·09 |
| IX. | – ·03 | ... | – ·03 |
| X. | – ·07 | ... | + ·02 |
| XI. | + ·01 | ... | – ·12 |
| XII. | + ·11 | ... | – ·04 |
| XIII. | – ·05 | ... | ·00 |
| XIV. | + ·04 | ... | + ·06 |
| XV. | – ·04 | ... | + ·01 |

This correction of – 0″·1 to the lunar equation results both from the right ascensions and from the declinations, and from each of the three lunations over which the observations extend, and points to the probable necessity for diminishing the presently accepted value of the moon's mass by nearly 1 per cent.

## On the Early History of some Scottish Mammals and Birds. By Professor Duns, D.D.

(Read February 20, 1893.)

(*Abstract.*)

This history presents many points of interest to the naturalist, the antiquary, and the historian. The paper is a plea for a wider, yet reliable, record and characterisation of these mammals and birds than we have at present. All zoologists are familiar with them, but the information consists mainly of details which none but specialists can appreciate. General readers look for more science, suggestive of habits and habitats. They wish to know something of the number of species recorded at different periods, their gradational relations, their geographical distribution, and the opinions of early observers as to them. Do their remains throw any light on their physical environments, or on man's condition at the time, as in touch with them, as influenced by them—the living among the living? In tracing the history of beast or bird, we meet with many collateral subjects of importance—such, for example, as climatal changes, alterations of surface over wide areas, links dropped out of gradational rank, and thereby the realisation of new conditions of existence by the removal of one old species or the introduction of one new species. Moreover, assuming the ever-active presence and power of innate elements of variation, we are concerned to know in what direction they have worked in the course of three or four centuries. Has the variation been only in form, in size, or colour? Or has it ever been, in the essentially species-element, "reproduction"? In seeking an answer to these queries, reference was made to stratigraphical and biotic methods of the classification of Surface Deposits, and to the importance of both when dealing with organic remains in superficial strata. When this paper was promised, it was intended to put on record notes on the remains of fifteen species of mammals and four species of birds, touching which we have early historic notices, and to exhibit a number of specimens in this connec-

tion, of much interest in themselves, and in the conditions associated with them. But the exhibition was delayed for a time.

With regard to these remains, the sources of earliest information are mainly two—literature and geology. With the former we associate Adamnan's *Vita Sancti Columbæ*, say, A.D. 697; Boece's *Scotorum Historia a Prima Gentis Origine*, A.D. 1526; Lesley's *De Origine Moribus et Rebis Gestis Scotorum*, A.D. 1578; and Sibbald's *Scotia Illustrata, sive Prodromus Historiæ Naturalis*, A.D. 1684. These works were characterised, and it was pointed out that they supply materials for a pretty full statement of the surroundings of the animals at the time when they were first referred to. They, and other works of the same class named, contain valuable information on subjects not likely to attract the attention of the specialist, but of much importance in the ancestral history of any one species.

In looking at organic remains from the point of view of the geology of the surface, much will depend on the qualifications of the observer. Conditions may be met with the right interpretation of which may have important bearings on our estimate of the remains of mammal or of bird associated with them. The influence of such considerations on questions connected with the geographical distribution and associations of some extinct and some early historic forms was discussed and illustrated, and the question of the contemporaneity of Man with several extinct mammals was referred to. These earliest records are interesting also from the point of view of the migration of mammals and birds. This was illustrated by reference to the remains of the reindeer (*Cervus tarandus*), found in Scottish superficial deposits, and to historic notice of the seasonal occurrence of the crane (*Grus communis*) in Scotland so early as the seventh century. Remains of the reindeer have been found in seven counties of Scotland, in England, Ireland, Belgium, and the South of France. The chief area of its distribution in Scotland seems to have been in the north. Do the remains met with in southern Scottish counties point to its seasonal wanderings?

As to the crane: Lesley, who distinguishes between the heron (*Ardea*), the stork (*Ciconia*), and the crane (*Grus*), says there were many cranes (*plurimi grues*) in Scotland in his day. It was thus at one time abundant seasonally. Now, however, not more than one or two in a century find a place in the ornithological record.

Reference was also made to the Great Bustard (*Otis tarda*) and to the Great Auk (*Alca impennis*) in connection with other subjects discussed in the paper. In conclusion, it was held that an adequate description of the environments, physical and vital, of the mammals and birds earliest recorded by Scottish observers would shed much light (1) on the physical features of the districts frequented by them at the time of their first record; (2) on the climatal condition of these districts; (3) on their fauna and flora; (4) on the value of the biotic element in schemes of classification of Surface Deposits; (5) on the phenomena of migration; (6) on the laws and the limits of variation; and (7) on some questions of ancestral history. Many of the materials for generalisation on these questions lie often at present in out-of-the-way records, and, to most, in recondite quarters, but they are worth being sought for and utilised as a branch of common culture, and, chiefly, as a great help to writers of civil history—help which can be given only by students in the departments to which reference has been made.

## On a Remarkable Glacier-Lake, formed by a Branch of the Hardanger-Jökul, near Eidfiörd, Norway. By Robert Munro, M.D., M.A. (With a Plate.)

(Read March 20, 1893.)

Lakes whose causal conditions are due to the direct interposition of glaciers, may be formed in one or other of the two following ways. First, when a glacier descending a lateral valley protrudes so far as to dam up a river in the main valley, the obstructing element being either the actual ice or the morainic débris deposited from it. Or, secondly, when a glacier occupying a main valley blocks up the outlet of some tributary stream. In both instances the water accumulates behind the obstruction and so forms a lake, varying in size according to the height of the barrier and the surface configuration of the district immediately above it. When the obstruction consists partially or wholly of ice, the stream issuing from the lake becomes liable to sudden inundations, as the pent up waters sometimes burst a passage through or underneath the ice, and so cause havoc among the industrial products of man along its whole downward course. A typical example of this kind of lake may now be witnessed in the Mattmark See, situated in the upper part of the Saas Valley, in Switzerland, where the Saaser Visp has become dammed up by a mound of débris carried into the valley by the Allalin glacier. This glacier has, of late years, been decreasing in size, and the ice itself no longer forms part of the dam, which is thus exclusively composed of the accumulated débris of the terminal moraine, over which the river is now gradually deepening its channel. At an earlier period, however, certainly not many centuries ago, the glacier extended right across the valley, a fact clearly shown by the fresh-looking morainic deposits containing fragments of "gabbro," which are to be seen high up on the opposite slope. That during this period the Mattmark See was subject to violent outbursts, may be inferred from the traditional dread of inundations which is still prevalent among the inhabitants, and finds expression in the numerous votive crosses exposed to view in convenient corners along the road in the Saas Valley.

Glacier-lakes, formed after the manner of the Mattmark See, are numerous, and are to be found not only in Switzerland but over the whole geographical area formerly occupied by glaciers. On the other hand, those resulting from the blocking up of the entrance to a tributary valley are comparatively rare, and only one or two examples are at the present time known to be in existence. Their comparative rarity is due to the fact that the obstructing dam is invariably composed of ice, and the effect of the lateral moraine, should any exist, may be disregarded. The continuance of the lake, therefore, depends entirely on the evanescent character of the glacier, and no lake remains after its disappearance.

The most noted glacier-lake of this kind, and the best known in Europe, is the Märjelen See, situated at the foot of the Æggischhorn, on the left bank of the Great Aletsch glacier. It lies in a small lateral valley which, at its upper end, communicates by a low "col," or pass, with the adjacent valley of the Viesch; and it is by this "col," and not by the glacier, that in ordinary circumstances its surplus water is discharged. If, however, in consequence of some accidental fissure, or other change in the ice, the water finds an escape in this direction, it does so with such rapidity that sometimes in a few hours the lake disappears altogether, so that nothing is to be seen in its former bed but some stranded masses of ice and a small stream at the bottom of the basin. The lake then begins to refill, and after about a year the water reaches its former level, at which it remains stationary till some other structural change in the ice-dam allows it to escape, and the phenomenon of its sudden drainage is repeated. The devastation usually caused by these floods on the lower lands along the banks of the Massa and the Rhone (into which the former falls), recently induced the interested landowners to deepen the outlet by the "col" so as to lessen the water capacity of the Märjelen See, and, of course, to proportionally diminish the damage from these periodically recurring inundations. The Märjelen See was visited in 1865 by Sir Charles Lyell, and its then condition is thus described in his *Principles of Geology*, 11th ed., p. 373:—"The Märjelen See was about 2 miles in circumference when I visited it in August 1865, and about 40 feet below its normal level; for in the month of June in the preceding year, it had undergone one of its periodical drainages, and the basin had not yet been filled again. Such a state of things gave me an opportunity of examining a point of great geological interest, namely, the form and structure of a large terrace or line of beach which encircles the lake-basin all round its margin, and which constitutes its shore when it is full, and when its surplus waters flow over to the Viesch Valley. I satisfied myself that this terrace is a counterpart of one of those ancient shelves or parallel roads, as they are called, of Glen Roy in Scotland, which, as Agassiz first suggested, were probably formed on the edge of lakes dammed up by ice, which may have existed in the glacial period in Scotland."

Professor Bonney, writing in *Nature*, October 27, 1887, describes the Märjelen See as "unique of its kind," and gives some interesting details in regard to the changes to which it is liable. Two years later, Prince Roland Bonaparte published a large pamphlet, entitled *Le Glacier de l'Aletsch et le lac de Märjelen*, in which he records a number of incidents and catastrophes consequent upon the sudden fluctuations of the lake, and also enumerates the dates of its periodical drainages so far as known historically.

The temporary lake formed by the advance of the Giétroz glacier in the upper part of the Dranse Valley, the bursting of which, in 1818, caused such devastation, seems to be, at least in some respects, analogous to the Märjelen See. But a still more perfect parallel to it is the one in Norway, which I am now about to describe.

My sole purpose in visiting Eidfiörd was to see the famous Vœringsfos, and having accomplished that object, I happened to notice that same evening a large photograph hanging in the corridor of the hotel, representing a glacier terminating in a lake. On making inquiry as to its situation, I ascertained that the glacier could not only be visited in one day, but that it had a history of such a novel character as to greatly increase my curiosity to see it. For the following particulars of the stories current, I am indebted to Mr Näsheim, proprietor of the Vœringsfos Hotel, who, as an intelligent native, took an interest in the history and traditions of the neighbourhood.

"The lake," said he, "represented by this photograph, lies on the high plateau immediately beyond the Rembidalsfos and the river which flows out of it, and traverses the whole length of the Simodal, is subject to violent inundations, which, at different times, have caused great loss to the inhabitants owning the lands along its course. Only three years ago one of these inundations occurred. It was about three o'clock in the morning when a great noise was heard, and the terror-stricken people rushed out of their beds to save themselves and their properties. Two fine bridges were at once swept away by the raging flood—one of which had been newly erected at a cost of 4000 kronors (£225). This bridge was just on the eve of being handed over to Government, but, as the formal ceremony had not taken place, the poor people had to reconstruct it at their own cost. The lands in the Simodal are divided into four sections, owned by twenty-four farmers, and the entire population amounts to about 100. Eighteen years ago another calamity of the same kind devastated the valley; and it has been the custom of the inhabitants from time immemorial to appoint special days for offering up prayers to God to preserve them from such misfortunes. Now, however, they have come to realise that prayers are more effectual when conjoined with material action, and so an engineer has been employed to suggest, if possible, some practical means of preventing the recurrence of these destructive inundations."

My informant then explained that these floods were caused by the bursting of a temporary lake, which forms every year during the spring and summer, in a valley at the side of the glacier. When this happened, the sudden rush of water flooded the lake below the glacier, and speedily raised its level, some 4 to 6 feet, above its normal condition, and, consequently, to the same extent, flooded the river in the Simodal. On inquiring why this flood did not take place every year, I was informed that, in ordinary circumstances, when the temporary lake-basin became filled up, the water flowed over the top of the glacier, and by degrees excavated a channel for itself through the ice, by means of which the lake became emptied in three or four weeks. At other times, however, the water forced a passage beneath the glacier, and in this case escaped in a very much shorter time; and the greatness of the flood depended upon the rapidity with which the lake became emptied. Among the measures proposed to rectify this state of matters was the construction of a dam across the outlet of the lower lake (Rembidalsvand), with a sluice to regulate the flow of water, so that on an emergency of this kind arising, only a certain quantity of water could escape in a given time.

At that time of the year (20th August) this temporary lake, as I was informed, had already disappeared; but notwithstanding this I was determined to visit the locality, in order to get as correct an idea as possible of the physical and topographical conditions which gave origin to such an unusual phenomenon.

The inner reaches of the great fiords on the west coast of Norway consist of a series of deep, narrow valleys, to which numerous streams from the upper plateaus and snowfields converge. These valleys, bounded by lofty precipices on both sides, ramify for miles into the interior of the country, and generally end in a kind of *cul-de-sac*, with a perpendicular frontage some 2000 or 3000 feet high.

Such a valley is the wild ravine known as the Simodal, which begins at the head of the Eidfiörd and terminates with the Rembidalsfos. After a journey of about four hours, the first portion of which was by a rowing boat, we reached this fine waterfall, whose roar and spray had already for some time attracted attention. It takes an hour's hard work for a steady walker to mount to the plateau above. This can be achieved by following a zigzag path among a mass of fallen rocks—the gradually-accumulated talus of post-glacial ages. Near the top one has to draw largely from his stock of mountaineering agility to climb up an overhanging precipice with the aid of a fixed rope and some roughly-cut footsteps in the rock.

The change from the confined valley below to the keen, clear atmosphere of this plateau, with its far-reaching views, was like coming into a new world. A walk of a few hundred yards over some rocky prominences and one or two treacherous bogs, still fed by the vanishing remnants of a snow-wreath, brought me to a commanding position, which afforded, for the first time, an opportunity of scanning the salient features of my surroundings. At my feet lay the lake whose beautifully clear water was as tranquil as if it were a sheet of glass. At its upper end, about a mile distant, and directly facing me, was the glacier which, in curving over a steep declivity, disclosed a charming view of its entire lower portion.

Owing to the action of the water, which, now and again, detached large masses of the ice, its lower end presented the appearance of an ice-cliff stretching across the lake. Here and there along the face of this cliff could be seen an arched cavern, whose dark recesses were in striking contrast with the greenish hue of the adjacent ice and the silvery whiteness of the great mass behind. On both sides of the glacier the rocks extended in elevated knolls, by no means comparable in effect to the grandeur of the lofty pinnacles, among which some of the Swiss glaciers meander. A little to the left of my point of view, and stretching away in the distance from the upper shore of the lake, was an expanse of mountain pasture on which a herd of cattle was browsing in the vicinity of a well-known Saeter. The lake itself occupied a rock-cut basin, and close by I could see prominent portions of rock protruding in rounded bosses, and bearing on their surfaces characteristic glacial markings. Some stranded boulders were also conspicuous objects. One, poised fantastically on the bare rock at a little distance, looked as if it were a good specimen of a rocking-stone. Moving for a few paces to the left, I came upon the outlet, which I inspected with an eye to its fitness for the proposed dam and sluice for regulating the outflow. It was a slight channel worn out of the solid rock, and hence it appeared to me to be eminently well adapted for such a purpose, and an adequate dam could be constructed at a small cost.

In now considering the ways and means of getting access to the glacier-lake basin in the higher regions, I soon realised the impracticability of my project for that day. Already the third post-meridian hour had passed, and the guide had no knowledge of the locality beyond the ice-cliffs. To make further progress it was necessary to signal to the people in the Saeter for the boat (only one being kept); and after getting by this means to the other end of the lake, it was necessary, judging from the map and the landscape before me, to climb some two miles along the steep and rocky side of the glacier. Nor was it a certainty that the great reservoir could be approached in this direction at all. Hence, for the remaining portion of my description, I have to rely to some extent on indirect evidence—evidence, however, which I consider trustworthy. It consists (1) of notes taken by myself of statements made to Mr Näsheim by natives who had visited this little frequented district;

and (2) of a descriptive article on the subject which recently appeared in the *Bergen News*.

The Rembidalsvand is a sheet of water, roughly oval in shape, extending in a north-eastern direction for rather more than a mile in length, and a maximum breadth of three-quarters of a mile (see sketch map). The glacier—an offshoot from the extensive snow-field known as the Hardanger-Jökul—comes down by a rocky bed, and finally terminates in the lake. About a mile and a half from its lower end, and on the right side, there rises, somewhat abruptly, the mountain called Lure-Nut (or Lure-Peak), which presents a steep projecting front to the glacier. Immediately to the east of this mountain there is the entrance to a deep narrow ravine, only about 200 yards wide at its mouth, but which runs northwards, with an irregular breadth, for a couple of miles or so. The direction of this ravine is thus at right angles to that of the movement of the glacier, and the ice rises in front of it in the form of a great dam. In the bed of this ravine are two small ponds, the upper of which is on a higher level, and is connected with the lower by a small waterfall. Above these ponds the ravine, which appears to be here less rocky, is drained by a small stream falling into the upper pond. The snowfield which feeds the glacier rises, in the form of a dreary circular plateau, from 9 to 10 miles in diameter, to the height of 6530 feet. The above-described ravine lies at an intermediate point along the route of the glacier, and has an elevation of about 5000 feet; but the exact height of its floor above the level of the Rembidalsvand I have not been able to determine with accuracy. It must, however, be considerable, amounting, at least, to several hundred feet. These topographical details will enable you to understand the following extracts from the article in the *Bergen News* already referred to:—

"In front of these pond-lakes the glacier pushes itself in a westward direction against the Lure-Peak, where it becomes broken np into immense fissures, and thence it continues its course to the Rembidalsvand, where large masses break away from it with such a frightful noise and splash as to be seen and heard from the adjacent Saeter. These masses of ice float in the lake during the whole summer, thus presenting in miniature the appearance of a polar sea with its icebergs.

"The ice-dam, which lies in front of the pond-lakes, is about 300 feet high, and, to this extent, it closes up the entrance to the valley in which they lie, and which extends northwards amid the rocks for about 4 kilometres. During the autumn and winter the ice becomes tight, partly on account of the movement of the glacier, and partly because the snow and ice freeze together. In this condition, when the spring thaw sets in, the water in the lowest pond-lake begins to rise and soon reaches the level of the upper, and the two become one lake. But still the water rises, and continues to do so till it finds an outlet over the surface of the glacier. A natural consequence of this rising of the water is, that large masses become detached from the glacier, because the ice adjacent to the Lure-Peak is greatly fissured. Icebergs from 400 to 500 feet in diameter are thus formed, which float about in the lower part of the lake, but only the smaller portions go far up, because the water there is shallower.

"As soon as the water reaches the top of the glacier it begins to clear a way for itself by excavating a channel through the ice, and it depends on the amount of ice the stream can melt each day how great its volume may be. In this way it takes three or four weeks to empty the valley of its contents, during which the Rembidalsfos is very fine, on account of the extra quantity of water in the river.

"This is the usual way in which the water accumulated in the valley of the pond-lakes finds an exit. But in certain years it forces a passage beneath the glacier, and, in this case, a dangerous flood ensues, because the force of the water is so great that nothing can stand in its way, and so the whole escapes in a few days. On such an occasion the portion of the glacier below the pond-lakes presents a most wonderful spectacle. The water is forced up through the numerous holes and fissures in the ice, and reappears on the surface of the glacier like so many geyser-fountains playing over it, because the pond-lakes, being on a much higher level, the pressure is very great, and the openings in the ice are small in comparison with the volume of water. It is thus a very uncommon sight to see the glacier, as it were, washing itself; and this is the reason that the ice of this glacier is so much cleaner and finer than that of any other. The Rembidals-glacier is, therefore, in the proper sense of the words, a glacier which has actually washed itself.

"After the pond-lakes are emptied another strange spectacle may be seen. The stranded icebergs lie pell-mell over the former bed of the lake; but some, clinging among the mountains, where they have found a slight fastening, come sooner or later tumbling down, and it is very dangerous to travel in their vicinity."

It would appear from the above descriptions that the Rembidalsvand serves, to a certain extent, as a safety-valve against the sudden rush of water, by distributing its volume over the entire area of the lake, as, without it, the flood in the Simodal would be a veritable debacle.

In a recent communication Mr Näsheim makes the following explanatory statement, which is more interesting because the subject had not been suggested to him by any queries on my part:—

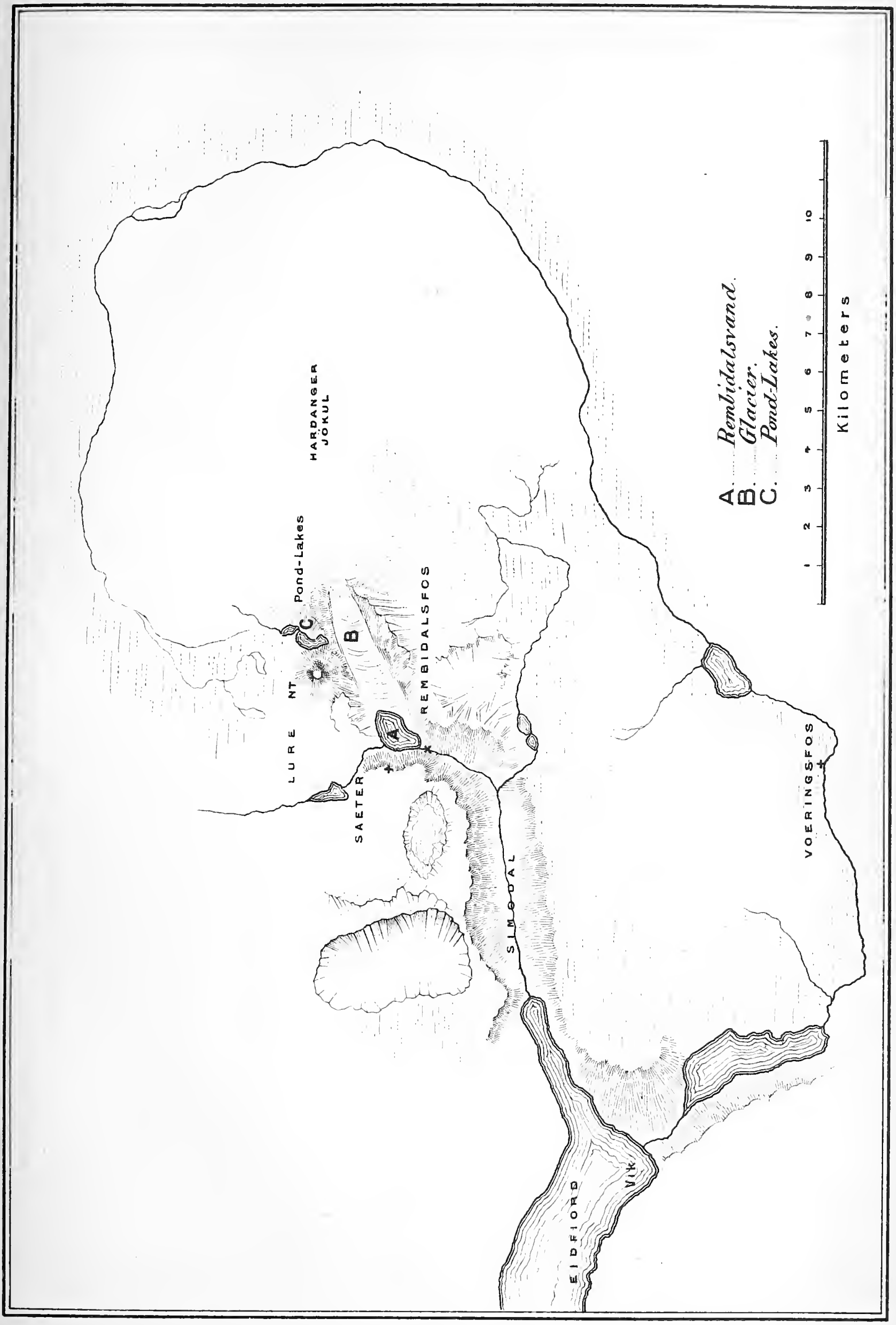
"You will not find the mountains so high at the north of the pond-lakes. I have spoken to people who have been there in the summer time, and they tell me that there are marks showing that the water from the pond-lakes has gone over the mountains on that side. Possibly this was the case when, at the end of the glacial period, the glacier blocking up the entrance to the valley of the pond-lakes had been much higher."

I am of opinion that the markings here referred to can be nothing else than the remains of an old lake-beach; and, if so, the phenomena are perfectly analogous to the "Parallel Roads" of Glenroy. But the point requires, as it certainly is worthy of, further investigation.

These are the principal facts in regard to this glacier-lake which I have been able to bring together, and I think them worthy of some record, inasmuch as they help to explain the conditions under which such lakes are formed and the physical effects they are capable of producing. They probably played a more important part, as geological agents, during the quaternary period, than is now generally suspected, more especially during its later stages, when all the streams and rivers were filled to overflowing with the excess of water from the dissolving glaciers. From this point of view it may be of interest to recall the evidence of the existence of glacier-lakes furnished by these so-called parallel "roads" of Glenroy by way of showing that it corresponds in its minutest details with the facts above described. The level of each of these "roads" coincides with that of a "col" in an adjacent valley, which, for a certain period, would be the ordinary outlet. That the confined waters of this lake occasionally burst their ice-barriers is suggested by some irregularly deposited débris of aqueous origin observed in the lower parts of Glen Spean. If this be an admitted fact, the exceptional irregularity of the deposits in question would be readily accounted for, as the escaped waters from the Glenroy glacier-lake would be sufficient to cause a much greater deluge than anything of the kind known in historical times. The descending flood, meeting with no lake-basin on the way to act as a temporary stop-gap to its destructive course, would re-sort the superficial deposits in the lower valley in such a way as to defy interpretation by the most skilled geologist, unless he were conversant with the exceptional glacial phenomena here faintly portrayed.

# DR. R. MUNRO ON A GLACIER LAKE, NEAR EIDFIORD, NORWAY.

SKETCH PLAN OF THE REMBIDALS-GLACIER AND LAKES




A. RITCHIE & SON, EDIN^R

## On the Compressibility of Liquids in connection with their Molecular Pressure. By Professor Tait.

(Read March 6, 1893.)

That liquids, if finitely compressible, must (at any one temperature) become steadily less compressible as the pressure is raised, seems to be obvious without any attempt at proof. Yet the assertion is even now generally made, mainly in consequence of an erroneous statement of Örsted, which has been supported by some comparatively recent investigations of Cailletet and others, that the compressibility of water (at any one temperature) is practically the same at all pressures not exceeding a few hundred atmospheres.

But in 1826 (*Phil. Trans.*, cvi.), Perkins had clearly established the fact that the compressibility of water at 10° C. diminishes:—rapidly at first, afterwards more and more slowly:—as the pressure is gradually raised. Perkins' estimate of his pressure-unit seems to have been considerably too small, so that his numerical data are not very trustworthy:—but this does not in the least invalidate the proof he gives of the gradual diminution of compressibility; for that depends of course upon relative, not upon absolute, values.

In the very earliest determinations which I made, some ten or twelve years ago, while examining the pressure-errors of the *Challenger* thermometers, this diminution of the compressibility of water was prominently shown:—and in 1888 I gave, as a fairly close approximation to the average compressibility for the first $p$ atmospheres, the empirical expression

$$A/(B+p),$$

in which the constants depend on temperature only.

This, it will be observed, is in complete agreement with the form of the result of Perkins. I also found that the addition of common salt, to the water operated on, had the effect of increasing the constant B in this formula by a quantity proportional to the amount of salt added; A being practically unchanged, so long as the temperature was kept constant.

These considerations seemed to point to the quantity B as being at least closely connected with the internal molecular pressure (usually named after Laplace); and, speculative as the idea confessedly is, it seemed worthy of further development. Another argument in its favour is furnished by a consequence of the hypothesis. For it is easy to see that when the average compressibility of a substance can be represented by the expression above, the equation of its isothermals must have the form

$$(B+p)(v-\alpha)=C;$$

approximately that given by the kinetic theory of a gas, when it is regarded as an assemblage of hard spherical particles.

Nearly three years ago, while I was preparing for press the second edition of my text-book "*Properties of Matter*," M. Amagat kindly gave me several unpublished numerical details of his magnificent experiments on the compressibility of water and ether. The following short table gives in its second column some of these results for water at 0° C. :—

| Pressure. | Volume. | *a* | | *b* | | *c* | |
|---|---|---|---|---|---|---|---|
| 1 | 1·00000 | 1·00000 | 0 | 1·00000 | 0 | 1·00000 | 0 |
| 501 | ·97668 | ·97664 | + 4 | ·97652 | +16 | ·97657 | +11 |
| 1001 | ·95645 | ·95662 | −17 | ·95644 | + 1 | ·95652 | − 7 |
| 1501 | ·93924 | ·93925 | − 1 | ·93909 | +15 | ·93916 | + 8 |
| 2001 | ·92393 | ·92405 | −12 | ·92393 | 0 | ·92399 | − 6 |
| 2501 | ·91065 | ·91064 | + 1 | ·91058 | + 7 | ·91062 | + 3 |
| 3001 | ·89869 | ·89870 | − 1 | ·89873 | − 4 | ·89875 | − 6 |

The numbers in the columns *a*, *b*, *c* are volumes calculated respectively from the following formulæ for the average compressibility for $p$ atmospheres :—

$$\frac{\cdot 30454}{6019+p}, \quad \frac{\cdot 30}{5887+p}, \quad \frac{\cdot 3015}{5933+p}.$$

The first was calculated from the data for 1, 1501, and 3001 atm.; the second from those for 1, 1001, and 2001 atm.; the third was obtained from them by interpolation. After the numbers in each column the difference "observed − calculated" is given. These are all small; and, especially in the case of formula *c*, the coincidence seems almost perfect throughout, for the differences have regular alternations of sign. But it is to be noticed that simultaneous increase, or diminution, of A and B by as much as 2 per cent. does not seriously affect the agreement of the formula with the results of experiment.

I have been for some time preparing to undertake an extended series of experiments on the compressibility of various aqueous solutions, with the view of finding (although by an exceedingly indirect and possibly questionable process) how the addition of a salt to water affects its internal pressure. But the recent publication of the final results of Amagat's experiments on the compression of water by pressures rising to 3000 atmospheres (more than six-fold the range attained in my own work) has led me to make a new series of calculations with the view of testing how far the above speculations, suggested by the results of pressures limited to some three tons' weight per square inch, are borne out by the results of pressures of twenty tons. The agreement, as will be seen, seems on the whole highly satisfactory; though, for a reason already given, and presently to be even more forcibly illustrated, the calculations are necessarily of a somewhat precarious character.

Thus we obtain from Amagat's paper (*Comptes Rendus*, 9/1/93) the following determinations of the volume of water at 0° C., for additional pressures of 400 and 800 atmospheres :—

| Pressure. | Table, No. 1. | Table, No. 2. |
|---|---|---|
| 1 | 1·00000 | 1·00000 |
| 401 | ·98067 | ·98071 |
| 801 | ·96371 | ·96371 |

The pressures in Table 1 extend to 1000 atm. only, those in Table 2 to 3000 atm.

These give, respectively, for the average compressibility of water per atmosphere for the first $p$ additional atmospheres, $p$ ranging from 0 to 800;

$$\frac{0{\cdot}296}{5725+p} \qquad \frac{0{\cdot}3057}{5939+p}$$

whence the compressibility at ordinary pressure may be either

$$0{\cdot}0000517 \quad \text{or} \quad 0{\cdot}00005147.$$

To enable us to choose between these formulæ we have the following comparison with the data for higher pressures in Amagat's second table :—

| Pressure. | Amagat. | First formula. | Second formula. |
|---|---|---|---|
| 1001 | ·95596 | ·95599 | ·95595 |
| 2001 | ·92367 | ·92337 | ·92299 |
| 3001 | ·89828 | ·89824 | ·89741 |

The first formula, therefore, represents with remarkable closeness the average compressibility of water at 0° C. for any range of pressure up to 3000 atmospheres; while the second obviously gives considerably too much compression at higher pressures. Yet there is but one numerical difference between the sets of data from which these two formulæ were derived, and that is merely a matter of four units in the fifth decimal place of the volume at 401 atmospheres! Thus very small inevitable errors in the data may largely affect the values of the constants in the formula. The only certain method of overcoming this difficulty would be to work with pressures of the same order as B.

The expression which I gave in 1888 for the average compressibility per atmosphere at 0° C. was (*Challenger Report*, *Physics and Chemistry*, Vol. ii. Part 4, p. 36)

$$\frac{0{\cdot}001863}{36+p},$$

the unit for $p$ being 1 ton weight per square inch. To atmospheres (152·3 per ton weight per square inch) this is

$$\frac{0{\cdot}284}{5483+p},$$

giving 0·0000518 as the compressibility at ordinary pressures. This agrees closely with the first, and more accurate, of the two formulæ just given; and yet it was derived from data ranging up to 450 atmospheres only. I stated at the time that "probably both of the constants in this formula ought to be somewhat larger." This would make it still more closely agree with Amagat's results.

I have worked out the values of the quantities A and B for the ten special temperatures (from 0° to 48°·95 C. inclusive) in Amagat's table No. 2; taking for each temperature the data for pressures 1, 1501, and 3001 atmospheres. The resulting formulæ give results agreeing very fairly with the compressions given for 501, 1001, 2001, and 2501 atmospheres:—the agreement being in fact almost perfect for the two higher pressures, but the compression being (as a rule) slightly in defect for the lower pressures. M. Amagat himself has stated that his results for lower pressures are given more accurately in the series of experiments where the pressure was never very great, than in those where it was pushed to 3000 atm. In fact his manometer had to be made considerably less sensitive when very great pressure was employed. For the reasons just pointed out I cannot wholly trust these calculations, and therefore I think it unnecessary to give them here. But they agree (with only one exception, for 29°·43 C.) in a very remarkable manner in showing that the values of A and B steadily *increase* with rise of temperature up to about 40° C., and thence apparently diminish. That the value of A should at first steadily increase with rise of temperature was of course to be expected as a consequence of the known change of molecular structure if (in accordance with the supposed analogy of the kinetic gas formula above quoted) it represents the utmost fractional diminution of volume which can be produced by unlimited pressure. And Canton's old discovery, that rise of temperature involves diminution of compressibility, requires that B should at first increase more rapidly than does A. [This is not necessarily inconsistent with the commonly received statement that the surface-tension of water is, in all cases, diminished by rise of temperature.] The turning-point seems to be connected with the temperature of minimum compressibility, discovered by Pagliani and Vincentini.

The behaviour of water at ordinary temperatures is of such an exceptional character that we cannot feel certain that aqueous solutions may not show more than mere traces of it. In my projected experiments, therefore, I intend to employ at least three different solutions of each of the salts to be examined, one of them being only a little below saturation strength. The comparison of the results for solutions of very different strength may enable me to eliminate the effects of the peculiarities of the solvent.

As a contrast to the behaviour of water, above discussed, I give some results for sulphuric ether; also founded on data furnished to me three years ago by M. Amagat. These data were given to four decimal places only.

| | 0° C. | | 20°·2 C. | |
|---|---|---|---|---|
| Pressure. | Amagat. | Formula. | Amagat. | Formula. |
| 1 | 1·0000 | 1·0000 | 1·0320 | 1·0320 |
| 501 | ·9468 | ·9498 | ·9673 | ·9722 |
| 1001 | ·9130 | ·9156 | ·9294 | ·9311 |
| 1501 | ·8884 | ·8885 | ·9018 | ·9018 |
| 2001 | ·8684 | ·8684 | ·8805 | ·8797 |
| 2501 | ·8522 | ·8524 | ·8630 | ·8624 |
| 3001 | ·8394 | ·8395 | ·8484 | ·8484 |

The agreement is not by any means so complete as in the case of water:—but it is probable that slight changes in the values of the constants may greatly improve it where defective, while otherwise scarcely interfering with it.

The formulæ for average compressibility employed were, respectively

$$\frac{\cdot 2863}{2350+p} \text{ for } 0^\circ, \quad \text{and} \quad \frac{\cdot 3016}{2086+p} \text{ for } 20^\circ \cdot 2 .$$

(Note that calculation from the data, direct, gives 0·31126 as the value of A in the second of these, but this has to be divided by the volume at one atmosphere.) Here, according to the previous mode of interpretation, the Laplace-pressure is diminished, and the ultimate volume seems to be increased by rise of temperature, as was to be expected.

## On certain Concretions from the Lower Coal Measures, and the Fossil Plants which they Contain. By H. B. Stocks, F.C.S. *Communicated by* John Murray, LL.D.

(Read May 1, 1893.)

Coal, we are all aware, is a remnant of the organic matter of plants which flourished long ago. Owing to the length of time that has elapsed since the remains were entombed, and the pressure the material has been subjected to by overlying strata of great thickness, coal now presents to the unaided eye little or no evidence of organic structure, but is a lustrous black product not at all like wood in appearance.

When thin sections of coal are examined microscopically, usually no trace of organic structure can be seen, but portions of plants, especially spore cases of lycopodiaceous origin, may be recognised in some specimens, the cells being considerably altered in appearance owing to the pressure and the chemical changes that have occurred in the deposits.

*Occurrence of Concretions in the Carboniferous Formations.*—In the carboniferous system, concretions abound; they are to be found in the millstone grit, in the coal, and in the accompanying shale. These concretions vary in appearance and composition according to the nature of the rock which encloses them; in the present paper I shall deal with those that occur in the coal only.

These particular concretions to which I refer occur in restricted areas, as at Halifax in Yorkshire and Oldham in Lancashire, the only two localities that I know of where these nodules are at present found. In these two districts they are known to the miners as "coal-balls," from their shape and outward appearance. (See specimens exhibited.)

Not only do the coal-balls occur locally, but they are found only in one particular *bed* of coal, known as the "hard-bed" seam, belonging to the Lower Coal Measures or Gannister Beds. It is probable that this seam is identical in both localities. The thickness of it varies from 2 feet to 2 feet 3 inches, and contains the nodules indiscriminately distributed, and not in any definite layers. This bed of coal lies upon an underclay, which in turn rests upon a sandstone, the latter being one of the upper members of the millstone grit formation.

Above the "hard-bed" seam is a bed of shale about 21 feet thick, full of the impressions of shells of undoubted marine genera, such as goniatities and aviculopecten.

The "coal-balls" vary in size from about 3 inches to 1 foot in their greatest diameter; they are usually ovoid, slightly flattened parallel to the strata, though occasionally spherical ones are found. Outwardly they have a black coaly appearance, derived from the matrix in which they are embedded, but when broken they present a brown or greyish-brown interior, often banded or streaked in various ways with bright masses of iron pyrites.

Up to the year 1883 no analyses of these concretions had been published, but in that year I read a paper before the Yorkshire Geological and Polytechnic Society and gave two analyses of them, which are here reproduced :—

*Analyses of Coal-Balls.*

| | *A.* | *B.* |
|---|---|---|
| Ferrous oxide, | 3·21 | 0·16 |
| Oxide of Manganese, | Trace | ... |
| Alumina | 0·33 | Trace |
| Lime, | 36·17 | 46·10 |
| Magnesia, | 0·88 | 0·30 |
| Silica, | 1·16 | 1·21 |
| Sulphuric acid, | 0·15 | 0·01 |
| Chlorine, | Trace | ... |
| Carbonic acid, | 29·00 | 35·28 |
| Phosphoric acid, | Trace | Trace |
| Iron pyrites, | 21·58 | 12·16 |
| Water, | 0·38 | 3·00 |
| Organic matter and loss, | 7·21 | 1·87 |
| | 100·00 | 100·00 |

The constituents are probably as follows :—

| | | |
|---|---|---|
| Carbonate of iron, | 6·00 | 0·30 |
| Alumina, | 0·33 | Trace |
| Carbonate of lime, | 64·41 | 82·32 |
| Carbonate of magnesia, | 1·82 | 0·61 |
| Silica, | 1·16 | 1·20 |
| Sulphate of lime, | 0·32 | 0·03 |
| Iron pyrites, | 21·58 | 12·16 |
| Hygroscopic water, | 0·25 | 3·00 |

A glance at these figures shows that the chief constituents are carbonate of lime and iron pyrites.

These concretions are exceedingly interesting because they usually contain remains of plants, petrified in such a wonderful manner that every cell is clear and well defined, and very different to the fragmentary portions found in the coal itself; they have therefore engaged the attention of local geologists and others, among whom may be mentioned Professor Williamson, the late Mr Binney, and Mr Spencer.

The portions of plants that are contained in these nodules are mostly large pieces of wood of species of Sigillaria and Lepidodendron; very often the whole nodule is one mass of fossil wood with a thin coating of unorganised mineral matter.

Several portions of fossil wood have been removed, and analysed with the following results :—

| | 1. | 2. | 3. | 4. |
|---|---|---|---|---|
| Ferrous oxide, | ·79 | 3·14 | 0·60 | 1·91 |
| Ferric oxide, | ... | 1·60 | 0·59 | 2·77 |
| Alumina, | ·01 | ... | ... | ... |
| Lime, | 48·32 | 18·05 | 49·61 | 30·49 |
| Magnesia, | 1·73 | 0·71 | 1·52 | 2·96 |
| Sulphuric acid, | Trace | 6·70 | 0·60 | 4·32 |
| Carbonic acid, | Not estimated | 11·80 | 39·09 | 26·80 |
| Silica, | ,, | 0·30 | 0·01 | 0·08 |
| Iron pyrites, | ,, | 48·63 | 4·75 | 24·25 |
| Organic matter, | ,, | 4·03 | 3·43 | 4·79 |
| Water and matter volatile at 110° C., | ,, | 4·25 | 0·25 | 1·61 |
| | | 99·21 | 100·45 | 100·38 |

The constituents are probably as follows :—

| | 1. | 2. | 3. | 4. |
|---|---|---|---|---|
| Carbonate of iron, . . | 1·44 | 5·77 | 1·12 | 3·57 |
| Ferric oxide, . . . | ... | 1·60 | 0·59 | 2·77 |
| Carbonate of lime, . . | 86·30 | 23·88 | 87·01 | 49·05 |
| Carbonate of magnesia, . | 3·63 | 1·49 | 3·19 | 6·21 |
| Calcium sulphate, . | Not estimated | 14·40 | 1·29 | 9·28 |
| Silica, . . . | " | 0·30 | 0·01 | 0·80 |
| Iron pyrites, . . | " | 48·63 | 4·75 | 24·25 |
| Organic matter, . | " | 4·03 | 3·43 | 4·79 |
| Hygroscopic water, . | " | 1·37 | ... | ... |

No. 1, of which only a partial analysis was made, was a specimen of *Sigillaria vasculare.* No. 2. Species not known, dark brown in colour, with a large quantity of iron pyrites running in parallel lines along the grain. No. 3. It was a specimen of Sigillaria, very hard, with no pyrites apparent. No. 4 was a specimen of *Lepidodendron Sternbergii*, black on the surface, brown in the interior, with much pyrites apparent.

The organic matter was determined in order that an idea might be formed of how much of the original substance of the plant remained, and in the three examples given the amounts were very close together; that is, about 4 per cent. In every case, on treating the specimen with hydrochloric acid, an odour of petroleum was perceived, and the residue contained fragments of but slightly altered cellular tissue, showing that the process of petrifaction was not complete.

Bischof, in his *Chemical and Physical Geology*, gives us a similar case to the above. He says :—" On treating a *Stigmaria fucoides*, fossilised by carbonate of lime from the transition formation, with diluted hydrochloric acid, Göeppert obtained a residue presenting the entire structure of the plant in its natural arrangement and colour. The wood of Coniferæ from transition rocks left only ·02 to 0·7 of feebly brown, perfectly flexible fibres together with some empyreumatic oil smelling like creosote."

*Suggestions as to Formation of the Coal-Balls.*—Mr Binney has stated (*Manchester Geological Society's Reports* and *Journal of the*

*Geological Society*, vol. xviii. p. 107) that the nodules in the coal are always associated with fossil shells in the roof or shale, the destruction of which has formed materials for the calcification of the nodules, by the solution and subsequent deposition of the carbonate of lime from the shells above around portions of the plants in the coal below. With this exception no theory has been offered to explain their formation.

An objection to this explanation lies in the fact, that the rate of deposition of strata is exceedingly slow, and that a considerable time would elapse before any appreciable thickness of mud with its shells could be formed; consequently in the interval the underlying plant substance must have undergone a considerable change and lost nearly all its original structure, whereas the plants enclosed in the nodules are as perfect now as when they grew in carboniferous times.

The plants forming the coal were deposited where they grew and have not been carried from a distance; the presence of the underclay containing rootlets of Stigmaria proves this; the overlying strata of shale containing marine remains also shows that the deposits were laid down in sea-water; and therefore we may adopt the general opinion that this particular bed of coal, like many others, has been formed in shallow water around a sea coast, as in the mangrove swamps of America at the present day. We may therefore trace the origin of the nodules to the matter dissolved in the sea water.

As in the mangrove swamps, owing to the matted state of the vegetation, there would be little or no interchange of the sea water, which would become loaded with dissolved organic matter to the exclusion of oxygen. When, therefore, a portion of a plant commenced to decay, the oxidation could only proceed at the expense of the soluble sulphates. Sea water contains only a comparatively small quantity of carbonate of lime, ·012 per cent. according to Dittmar, whereas sulphate of lime is present to the extent of ·126 per cent. Water containing calcium sulphate and other salts in solution easily passes through the vegetable membranes by osmosis, and coming in contact with the decaying organic matter of the cells, the calcium sulphate has been reduced to calcium sulphide with the liberation of carbonic acid. The latter immediately reacts upon calcium sulphide, producing calcium carbonate in the interior of the cells, where it replaces the organic matter molecule by molecule, sulphuretted hydrogen being set free. The sulphuretted hydrogen afterwards unites with iron, forming the compound known as iron pyrites.[1]

The reactions may be represented as follows:—

$$CaSO_4 + 2C = CaS + 2CO_2 .$$
$$CaS + CO_2 + H_2O = CaCO_3 + H_2S .$$

For each equivalent of carbonate of lime deposited we should accordingly have one equivalent of carbonic acid passing off as gas, provided the reaction was complete; but, under the circumstances no doubt, hydrocarbons were also formed, as we see in our peat bogs of the present day, small quantities of these hydrocarbons being found in the fossils after treatment with hydrochloric acid.

Sea water, therefore, accounts for the chief constituents of these nodules. The explanation I have above given is contrary to the opinion of Mr Binney, as it does not presuppose any interval of time to elapse between the processes of decay and petrifaction. The decomposition and replacement by carbonate of lime once having commenced at a particular place in the plant, the physical force known as aggregation would in time lead to the petrifaction of a large mass of it in the immediate neighbourhood, and decomposition still proceeding, amorphous carbonate of lime would be deposited around the petrified portion of the plant, when decay and fossilisation would be practically complete, and the nodule in a perfect condition.

I am led to think, from the slow rate of petrifaction, that decay and fossilisation proceeded simultaneously; moreover, the perfect nature of these enclosures, as may be seen when a thin section is prepared and examined microscopically, leads us to the conclusion that little or no decay had taken place before the mineral commenced to replace the organic matter; signs of decay would have been readily apparent had the trees lain for only a short time at the bottom of the water.

After a portion of the plant had become fossilised, the decom-

[1] For formation of iron pyrites in deep-sea deposits, see *Deep-Sea Deposits, Challenger Report*, by Murray and Renard, pp. 254–258.

position still proceeding would lead to the formation of carbonate of lime upon the outside of the parts, and thus the outer markings, being covered, are also protected; subsequently the unpetrified remainder of the plant decayed away, leaving broken ends upon which layer upon layer of mineral matter would form, each layer tending to bring the mass nearer to a spherical shape, until finally all decomposition in the wood ceased, and the concretion had attained its present size. There is no evidence that these nodules have grown in size since the time the coal was formed.

The reason for the mineral matter aggregating in the form of spheroids lies in the fact that when carbonate of lime is deposited slowly from a solution containing much organic matter in solution, it is prevented from assuming the crystalline character, but owing to the physical conditions, it assumes a more or less spherical appearance, as we see in the present case. As an example of this I quote the experiment of Mr George Rainey, mentioned in Hogg, *On the Microscope*, ed. 1854, who found that when a solution of calcium chloride and a solution of sodium carbonate, each in gum water of the same specific gravity, were carefully placed in a bottle so that mixture did not take place immediately, small spherules of carbonate of lime began to form and grow by the gradual diffusion of the liquids.

The process of petrifaction and the formation of these concretions is a difficult subject to deal with, seeing that our knowledge is as yet only limited. I have not been able to find any recorded theory apart from accretion to account for their formation; very little has up to the present been written upon petrifaction, although it is a subject of great interest. We have in these nodules specimens of plants showing perfectly every detail, not only external, but internal, and the question is,—How were they preserved in such a beautiful manner for ages? In the opinion I have expressed will be found an answer; and, moreover, I have a number of specimens of plants under treatment with a view to test the truth of the theory.

## On some Observations made without a Dust Counter on the Hazing Effect of Atmospheric Dust. By John Aitken, Esq., F.R.S. (With a Plate.)

(Read January 30, 1893.)

There was considerable difficulty in selecting a proper title for this paper. It may appear to some, after reading it, that a shorter and better title would have been, "On the Hazing Effects of Smoke." But as smoke particles generally reflect a reddish-brown light, they do not therefore seem to be the cause of the whitish light which we call haze ; and further, smoke particles generally condense into little masses of such a size that they fall to the ground before they are carried to any great distance. It was thought that as this paper is a continuation of previous work, and is founded on results already communicated to this Society, it would be better to keep to the use of the same terms, and as usual, under the name of "dust particles," to include all the solid and liquid products of combustion, of whatever size or colour they may be.

Although the results obtained in the investigation have been arrived at without the use of a dust counter, yet the work previously done by that instrument has been made use of. Indeed, without a knowledge of the relations which the dust counter has shown to exist between the thickness of a haze and the dust and humidity existing at the time, this investigation would not have been possible. The results contained in this paper prove that, without counting the number of dust particles, we can show that the transparency of the atmosphere is very much destroyed by the impurities communicated to it while passing over the inhabited areas of the country. As we know, from observations made with the dust counter, that the number of dust particles is great in air coming from these areas, and that the transparency of the atmosphere varies with the amount of dust in it, it may be assumed that this thickness is due to the dusty impurity communicated to the air by the products of combustion and other causes.

Shortly after beginning the observation of the number of dust particles in the atmosphere, I was discussing with a friend, who took considerable interest in the matter, the impurity of the air near Falkirk, and regretting the impossibility of making, near that place, any observations on dust which could be of any value from a meteorological point of view, owing to the vast amount of pollution thrown into the atmosphere by large towns such as Glasgow and Edinburgh, and the densely inhabited districts by which Falkirk is surrounded. My friend seemed somewhat astonished, and put the question—"But is it possible that Glasgow smoke can travel all that distance, about 20 miles?" To which the reply was, that there seemed to be no doubt about it, as the dust counter showed the air to be always very impure when the wind blew from densely inhabited areas. There was, however, always the possibility that the high numbers observed near Falkirk might have been due to local causes, though with the wind in some directions it could not have been so, as the area from S.E. to S.W. of Falkirk is very thinly populated for some miles southwards. This conversation turned my attention to devising means for testing whether the smoky impurities from towns and inhabited areas were carried to any great distance, and had any appreciable influence in thickening the air to the leeward of them.

On considering this question it became evident that by making observations on the haze in the atmosphere at different times, and noting the direction of the wind and the humidity of the air at the time, an answer might be got without the use of a dust counter. Working in this way, all uncertainty as to the effects of the immediate local pollution would be got rid of, as these only affect the lower air, while the haze is measured higher up.

In previous communications on the number of dust particles in the atmosphere, it has been shown that the thickness of a haze depends on the number of dust particles present, on the degree of saturation of the air, and to some degree also on the vapour pressure, —that is, on the absolute as well as on the relative humidity. Supposing we have two samples of air both of the same absolute and relative humidities,—that is, both at the same temperature, and both giving the same depression of the wet bulb thermometer,— if one of these samples be more hazed than the other, it will be found to have more dust particles in it than the other, and to be the thicker the greater the number of particles present. And for a given number of particles, the damper the air the thicker is the haze. There are probably other causes of haze, but those referred to are the principal ones.

The above conclusions place in our hands a means of comparing the amount of dusty impurity in different masses of air, or of different airs brought to us by winds from different directions. If we knew nothing about the humidity of the air, observations on the thickness of the haze alone would tell us nothing. But if we know the humidity, we are a step nearer knowing the amount of dust. For instance, if we find that on two particular days the wind was from different directions, that the wet and dry bulb thermometers showed the same difference, but that the air from one direction was more hazed than that from the other, then we may conclude that in the air with most haze there was most dust. Though this method of working tells us nothing about the actual number of particles present in the different airs, yet it enables us to compare the relative purity of different masses of air, or of airs coming from different directions. It is further evident that by this method of working we must compare the haze only on days when the humidities are the same. It, however, seems possible, after we have accumulated a sufficient number of observations, that we may be able to estimate the effect of the humidity, and we may thus be enabled to compare days when the humidities are not equal.

Adopting the above idea, observations were made to see if this method of working would give us any information as to what the hazing effect is of the products of combustion from the densely populated parts of our country. These observations were begun in June 1891, and have been continued to the end of 1892. The observations are not continuous, but were made from time to time as opportunity offered, and while I was at Falkirk, as all the observations for this investigation must be made at one place. In the note-book in which the records were kept, the date and hour are entered in the first two columns. In the next two the direction and force of the wind, then the temperature of the wet and dry bulb thermometers, after which were three columns for entering the amount of haze on three hills at different distances, and last there was a column for remarks.

For an investigation such as this Falkirk is particularly well situated.

By examining a map of Scotland, it will be seen that Falkirk lies a little to the north of a line drawn between Edinburgh and Glasgow, and is nearly midway between them. If we draw a line due west from Falkirk, and another due north, we shall find that in the north-west quadrant so inclosed the population of that part of Scotland is extremely thin, the country over that area being chiefly mountainous, and there is not a town in it of any size within 70 miles, with the exception of Stirling. In all other directions outside the N.W. quadrant the conditions are quite different. In the north-east quadrant are the fairly well populated areas of Aberdeenshire, Forfarshire, and the thickly populated counties of Fife and Clackmannan. In the south-east quadrant are situated Edinburgh and the well populated districts of the south-east of Scotland, and in the south-west quadrant are Glasgow and the large manufacturing towns which surround it. It will be thus seen that Falkirk is surrounded on three out of the four directions by thickly populated areas, while the fourth is very thinly populated. The result of this is, that while the winds from the N.E., S.E., and S.W. quadrants bring us air polluted in its passage over populated areas, the winds from the N.W. quadrant come to us comparatively pure. If, now, we compare the air that comes to us from those different directions in the manner already described, we shall see what the effect is of the products of combustion on the clearness of our atmosphere.

While Falkirk is particularly well situated with regard to its position relatively to the pure and impure surroundings, being situated at the apex of the pure N.W. quadrant, it is yet rather unfortunately situated with regard to the conditions necessary for estimating the amount of haze in the atmosphere. As in previous observations, the haze has been estimated by noting the most distant hill that could be seen through the haze. The distance in miles of the furthest away hill visible is then called the "limit of visibility" of the air at the time. But as it is almost never possible to get a sufficient number of hills at different distances to work in this way, the limit of visibility has generally to be arrived at by estimating the amount of haze on some hill at a known distance, and calculating from that estimate the greatest distance at which a hill could be seen under the conditions, if such a hill were in existence. For the observations made at Falkirk only three hills are available: one a low hill about four miles distant, situated a little north of west; second, the Ochils, at a distance of from 12 to 15 miles to the north; and third, Ben Ledi, at a distance of 25 miles to the N.W. It is fortunate that a hill so far away as 25 miles is visible, but the great defect of this situation for observing haze is that the view is open only in a northerly direction from N.W. to N.E. In all other directions the ground rises and shuts out the distant view. The great disadvantage of this is, that while all estimates of haze should be made in the line of the wind, either up or down it, so as always to look through air coming from the same area, at Falkirk the limit of visibility of all clear air has to be estimated on the haze observed on Ben Ledi—that is, looking in a N.W. direction. So that, supposing the wind at the time is from S.W., we shall be looking *across* the direction of the wind, and though the observer may be surrounded by impure air, that impure air may not extend to Ben Ledi, which receives its air from the pure area to the west of Greenock when the wind is from the S.W. This evidently will give rise to an over-estimate of the purity of the air coming from the westerly part of the S.W. quadrant.

As the wind gradually veers from S.W. to W., the northern limit of the impure air gradually comes nearer Falkirk; and when the wind has gone due W., all impure air passes to the south of it. This gradual approach of the northern limit of the impure air has been frequently observed, and may be seen whenever the wind veers from S.W. to W., if the air be at all dry. On looking N.W., Ben Ledi often becomes visible before the Ochils are seen, though it is further away, but it is seen through a shorter distance of impure air. After the Ochils have become visible, and while the wind is still south of west, the air to the N.E. is still very much hazed. The reason for this difference is, that while looking at the Ochils we see them across a short length of impure air between us and the purer air beyond, while in looking N.E. we are seeing through a greater length of the impure air. It is therefore evident that the estimates made of the haze in S.W. and particularly in W.S.W. winds must be too small, and the limit of visibility given in the tables too great. It seems also probable, for the same reason, that the estimates of the haze in N.E. winds are also too small.

The boundary between the pure air from the uninhabited and the impure from the densely inhabited districts is often very marked. On one occasion when I was on the Denny Hills, situated to the west of Falkirk, while there was a westerly wind blowing, the view to the south through the impure air was limited to a few miles, while to the north it was so clear that the most distant mountains were seen through but little haze.

When the air is thick, only the hill at four miles distance can be seen, then the Ochils become visible as the air clears, and at last Ben Ledi is seen when the haze becomes still less. Although this is not always the order in which the different hills become visible when the air clears, as has been already stated, yet it is the most frequent. After Ben Ledi is visible, it then becomes necessary to estimate the amount of haze on it, in order to get what we have called the limit of visibility of the air at the time. Thus, suppose Ben Ledi seemed to be half-hazed, then the limit of visibility will be 50 miles. In this way all the estimates of haze have been reduced to one scale for comparison.

After going over all the observations entered in the note-book, and rejecting all those which were unsatisfactory, from the conditions being uncertain from want of wind, wet ground, showery weather, &c., there remained a little over 200 observations, which have been classified and arranged for the purpose we have at present in view. As already stated, if on two days the humidity was the same, but on one day the haze was thicker than the other, then we may conclude that the greater thickness was due to a greater amount of dust in the air. But if the air on one of the days was damper than on the other, then the increased density of the haze may have been due to the increased humidity. From this we see that we must compare the density of the haze only on those days when the humidity was the same. The first thing, therefore, which had to be done with the observations was to arrange them all in tables according to the wet bulb depression at the time. Tables were accordingly prepared, in one of which all the observations made when the wet bulb depression was 2° were entered, while in other tables were entered the observations when the depressions were respectively 3°, 4°, 5°, 6°, 7°, and 8° and over. The different observations in each table were at the same time entered in such a manner that all those made when the wind was N. were together, all those when it was N.E. next each other, and so on. So that in each of the seven tables for the different degrees of wet bulb depression were entered the direction of the wind, and the limit of visibility on the different occasions, all those for each direction of wind being next each other.

It is unnecessary to give the whole of these observations, but the abstract given in Table No. I. may be useful. In the first column of the table is given the direction of the wind, and it applies to all the other columns in the table. In the next two columns are the observations made when the wet bulb depression was 2°. In the first of these two columns is entered the number of observations in which the direction of the wind was that shown in the first column when the wet bulb depression was 2°, and in the second is entered the limit of visibility, being the mean of all the observations, the number of which is given in the previous column. In the next two columns are entered in a similar manner the number of observations and the limit of visibility when the wet bulb depression was 3°, and in the other columns the number of observations and the limit of visibility when the depressions were respectively 4°, 5°, 6°, 7°, and 8° and over. The number of observations taken when the wet bulb depression was over 8° was very small; they have therefore been added to the 8° depression, as they do not appreciably affect the result.

If we examine Table No. I. we shall find confirmation of some points with which we are already acquainted from the observations made with the dust counter. It will be noticed that as the dryness of the air increases, the limit of visibility also increases. This is the case with the winds from all directions except from the N.E. When the wet bulb depression was 2° the E. wind had a limit of 10 miles, and increased to 22 miles when the air was dry enough to give a wet bulb depression of 8°. The S. wind increased from 8 to 32 miles, the S.W. from 7 to 17 miles, the W. from 50 to 172 miles, for the same increase in dryness. The failure of the N.E. winds to follow the general law may be due to the figures not being correct, owing to the small number of observations obtained when the wind was in that direction. It will be seen that there was only one observation with 5° of wet bulb depression, and one observation when there was 6°,—far too few to give a result that can be relied upon.

Another point which is very evident in Table No. I. is the great difference in the transparency of the wind from the different directions. With W., N.W., and N. winds the air is very clear, whereas from all the other directions it is very much hazed. All winds from E. by S. to S.W. are nearly ten times more hazed than those from the N.W. quadrant. Table No. II. shows this very clearly. In this table will be found, as in the previous one, vertical columns, in which are entered the observations for the different depressions of the wet bulb thermometer; in this table are given, in the top row of figures, the *mean* limit of visibility for all the winds from E. to S. and S.W. for each degree of wet bulb depression, and in the lower row of figures is given the mean limit of all the observations when the wind was in the N.W. quadrant. In this table it has been thought advisable to omit all the observations taken when the wind was N.N.E., N.E., and E.N.E., as well as those when it was W.S.W., because winds from these directions are not entirely from either polluted or unpolluted areas, and also because the observations of haze in winds from these directions are unsatisfactory, for the reasons already given. The figures in Table No. II. show that when the wet bulb depression is 2°, the air from the N.W. quadrant is about 6·2 times clearer than the air coming from the other directions; and that when the air was drier than gave more than 2° depression, the mean of all the observations shows that the air from the N.W. quadrant was more than 9 times clearer than that from the other directions. That is, the table shows us that the air from densely inhabited areas is so polluted that it is fully nine times more hazed than the air that comes to us from the thinly inhabited districts. Allowing for some pollution that must be thrown into our atmosphere in its passage over the N.W. quadrant, we may say that, in a rough way, when the wind is easterly or southerly, the atmosphere at Falkirk is about ten times thicker than it would be if there were no fires and no inhabitants.

It will be noticed that by the method of working adopted in this investigation the effects of the moisture in hazing our atmosphere have been eliminated, and only those due to the number of dust particles are shown. It should, however, be noticed that if these observations had been treated in a manner in strict conformity with our knowledge of the effect of moisture in causing haze, we ought to have divided them into different series according to the reading of the dry bulb thermometer at the time, because it has been shown in previous communications that there is good reason for supposing that for a given wet bulb depression the hazing is greater at high than at low temperatures. The observations ought, therefore, to have been arranged in tables according to the temperature at the time. But as the number of observations are so few, it was thought better to treat them in one series, as if temperature had no effect.

Table No. I. gives only the *mean* limit of visibility of a number of observations for each direction of wind; it may therefore be interesting to show how much the individual observations differed from each other. For this purpose Table No. III. is given. In the vertical columns, under the different wet bulb depressions, are entered the least and greatest limits of visibility observed for the different winds entered in the first column. This and the other tables are very incomplete, owing to there being no observations of some winds at certain degrees of humidity. It will be noticed from Table III. that the limit varies considerably for the same wind at the same humidity. This is what might have been expected, because it has already been shown by the observations with the dust counter that the number of particles varies greatly in winds from the same direction but at different times. As this variation takes place even in winds coming from what we call unpolluted areas, it will also take place in that coming from inhabited districts, owing to rise and fall of wind, changes in the state of trade, season of the year, &c., requiring more or less coal to be burned at one time than another. The limits given in Table III. do not seem to be greater than what might have been expected, nor are they greater than the dust counter has shown the number of particles to vary.

As already stated, the limit of visibility increases as the air gets drier. This is seen when we look along any of the horizontal lines of figures given in Table No. I. In Table No. II., where the observations are still further condensed, the mean limit of visibility rises rather more regularly with the decrease in the humidity than in Table No. I. It will be further noticed that, as a general result, the transparency of the air increases about 3·7 times for an increase in dryness from 2° to 8° of wet bulb depression. The limit of visibility ought probably to rise regularly with the decrease in the humidity, which it does not seem to do from the tables. But, before tables such as those given can show satisfactory results, it is evident we must have far more observations, in order to get the correct means. For instance, take the case of the south wind, the limit for 3° wet bulb depression rests on one observation, while that for 4° is the mean of four observations; and so on all through the tables. Though 200 observations may be a fair number, yet, as there are sixteen directions of wind which should be entered under seven different degrees of humidity,—that is, 112 points to be determined,—it is evident the observations are far too few to fix so many points. It would appear, however, that 200 observations, though far too few to give satisfactory results, yet when worked out they point to conclusions which agree fairly well with each other, and with previous observations with the dust counter.

It may be thought by some that the highest limit of visibility given in the tables is too great; that 250 miles is too great a distance for a mountain to be visible—that is, supposing it was above the horizon. This estimate has been made, as already explained, by estimating the haze on Ben Ledi, 25 miles distant from the place of observation; and as this mountain was occasionally estimated to be only $\frac{1}{10}$ hazed, that gives 250 miles as the limit on these occasions. I may also add, that when observing on the Rigi Kulm I have often seen Hochgorrach so clear that it did not look more than $\frac{1}{4}$ or $\frac{1}{5}$ hazed. Now, as that mountain is 70 miles distant from the Rigi, it makes the limit of visibility on those occasions about 300 miles.

There is another and perhaps a more striking way of showing the results of these observations by means of what might be called isatmid* lines. These are shown in the Plate which I have prepared for the purpose. As will be seen, these lines are drawn over a map of Scotland; and to show the relation between these lines and the density of the population, the map is divided into counties, and the counties shaded according to the density of their population, the darkest being the counties in which there are most inhabitants per square mile. The density of the population is indicated by the scale on the map. These isatmid curves remind one of isothermal and isobaric lines. They are, however, drawn on quite different

* Isos = equal; *atmis* = smoke, steam, vapour.

principles. The isatmid lines show the transparency of the air for winds from different directions and for different degrees of humidity. It must also be clearly understood that this map only refers to Falkirk, or some place near it. Every place has its own isatmid chart, the lines of which are determined by the density of the population surrounding it and the direction in which the populated areas are situated.

The curves in the Plate were drawn in the following manner:— Lines representing the directions of the wind were drawn through the round black spot representing the position of Falkirk on the map. On the lines so drawn were marked off in miles, using the same scale as the map, the mean limit of visibility of the winds from the different directions when the wet bulb depression was 2°, the figures being taken from Table No. IV. Through the points so obtained the isatmid line for 2° was drawn. The same thing was then done for the observations at the other wet bulb depressions. This has not been done for each degree of difference of wet bulb depression, but only for every 2 degrees. If we had used the figures in Table No. I. for drawing the isatmid lines, we should have obtained a rather unsatisfactory and somewhat confusing set of lines. As will be seen from the figures, some of the curves would have crossed each other, owing to the variation in the limit of visibility not rising regularly with the dryness, though it is in the highest degree probable that it ought to do so. This crossing is probably due to there being too few observations to give a good *mean*, and also to 1° of difference in wet bulb depression being rather small to give a decided difference in the transparency. A much better result is obtained by combining the figures for two degrees of wet bulb depression. This has been done, and Table IV. is the result of condensing the observations in Table No. I. In Table IV. the first column gives, as in the other tables, the direction of wind, the next two columns the number of observations and the limit of visibility when the wet bulb depression was 2°; in the next two columns are entered the number of observations and the limit of visibility when the depression was 3° and 4°; in the next two columns are combined the observations when the depression was 5° and 6°; and in the last two the observations when it was 7° and 8°. If we now examine the figures in this table we shall find that, with the exception of the N.E. and S.W. winds, all the others show an increase in the limit of visibility with increase of dryness. In the two exceptions referred to, each has only one figure not in accordance with the law, and both of these only show a very slight deviation from it.

In place of using the figures in Table No. IV. for drawing the isatmid lines, perhaps a more correct result might have been obtained by manipulating the figures still further, either by plotting on paper the limit of visibility for each direction of wind for increasing dryness, and drawing an easy curve through the points, and using the limits so obtained for drawing the isatmid lines. No doubt in this way a more finished set of isatmid lines would have been obtained, and the Plate would have been more pleasant to look at; but it is not thought that the number of observations is sufficient to justify the finished and final appearance which this process would give to the curves. The curves have therefore been drawn from the figures as they stand in Table No. IV., and they correctly indicate an unfinished state of matters.

It may be as well to point out the difference between these isatmid lines and isobaric or isothermal lines. While the latter represent the position of uniform pressure or temperature at a particular hour, the former do not represent the condition at any particular time; in fact, they represent conditions which never have an existence at one and the same time. An isatmid line for any particular hour would be a circle of a certain radius, described round the place of observation as a centre, on the supposition that no change took place in the air from the time it entered within the circle till the time it left it. In order, further, that the isatmid line should be a circle, we would also require to neglect the position of the sun relatively to the direction of observation. The isatmid lines represent the transparency of the air, not at any particular time, but of the different winds at different degrees of dryness.

The observer is supposed to be standing at Falkirk and to be looking directly towards the point from which the wind is blowing, and the isatmid lines show the mean limit of visibility for air coming from that direction at different humidities. Suppose, for instance, that at the time the wind was from the N.W., and that the wet bulb showed a depression of between 3° and 4°. Then the point where the line of vision—that is, a line drawn N.W.—is cut by the isatmid line for 3° and 4° on the map, gives the mean limit of visibility for air of that dryness coming from that direction. In this case it is 123 miles. If a circle be now drawn through this point, with Falkirk as a centre, the circle so described will be the isatmid line at the time of observation. Or supposing the wind to be S.W. and dry enough to give a wet bulb depression of 7° or 8°, then the S.W. line is cut by the 7° and 8° isatmid at a distance of 21 miles from Falkirk, and a circle of 21 miles radius described round Falkirk would be the isatmid line for the hour.

The shading in the Plate representing the distribution of population of Scotland and the isatmid lines for Falkirk show in a very clear way the relation between the density of the population and the purity of the atmosphere. The air brought by the winds from all the densely inhabited parts being very impure, as will be seen when the wind is from these parts, the isatmid lines shrink and draw close to Falkirk, and only separate and expand when the air comes from thinly populated areas, the shading and the area enclosed by the isatmid lines being complementary; as the shading deepens the lines draw close to Falkirk, and *vice versa.*

In the Plate the counties, as already stated, are shaded in proportion to their population, and the different counties have generally been treated as if the population were evenly distributed, the counties being shown shaded equally all over. This evidently does not correctly represent the true condition, especially in the counties of Edinburgh, Lanark, and Renfrew, in which there are large towns. As, however, these counties are some distance from Falkirk, this method of representing the population, and consequent impurity of the atmosphere, does well enough; but when we come to the shading of the areas near the place of observation, this method of representation, owing to the peculiar distribution of the population existing near Falkirk, would give erroneous impressions, and the representation of the population has to be more particularly localised. Stirlingshire, in which Falkirk is situated, and Dumbartonshire, situated immediately to the west of Stirlingshire, have both a very uneven distribution of population, which it is necessary to represent on the map to enable us to understand the results of the observations.

If we draw a line due west from Falkirk, we shall find that the part of Stirlingshire north of this line is very thinly populated, with the exception of the area near and surrounding the town of Stirling. The other populous parts of the county are near Falkirk, principally to the north, east, and south of the point of observation, and in the area south of the west line are Campsie, Kilsyth, &c.; while in the centre of the county are situated the Campsie Fells, the Kilsyth and Denny hills, and in the north part are the hills to the east of Loch Lomond, so that all north of the line drawn westwards through Falkirk, with the exception of the small area near the town of Stirling, the county is but thinly populated. For this reason, in the Plate the population of Stirlingshire is shown in proportion to its density at different parts.

If the line drawn westward from Falkirk through Stirlingshire be continued through Dumbartonshire, we shall find that almost the whole population of that county also is condensed into the area south of this line. In five of the towns situated to the south of this line, including Helensburgh, which is almost on the line, there are 64,698 inhabitants, or more than two-thirds of the whole population of the county. And if we were to add to the above number the population of the small towns south of the line, we should find that all the area north of it is extremely thinly populated. From this we see that the whole area of Scotland north of the line drawn due west through Falkirk is very thinly populated. If we had represented on the map the population of Stirlingshire and Dumbartonshire equally distributed over them, it would have been difficult to understand from such a map the great clearness of the west winds, as darkish areas would have extended some distance to the north of west. But by shading these counties in accordance with the distribution of the population, we see that the west winds at Falkirk do not blow over any areas but those that are thinly populated.

It will be noticed that the isatmid lines show a decided tendency to open out to the southward. If we examine a map of Scotland, it will be seen that the area from S.E. to S.W. of Falkirk is very much less densely populated than the areas to E. and W. of it. If the distribution of the population in the counties south of Falkirk had been represented in the Plate more in detail, a comparatively slightly shaded area would have extended over a considerable district to the south of Falkirk. The approach of the isatmid lines on the east is probably due to the impurities brought from

Edinburgh and the surrounding towns, as they lie in that direction; and the contracting of the lines to the S.W. is probably due to the impurities from Glasgow and the surrounding manufacturing towns, as they lie to the south-west of Falkirk. The position of Edinburgh and Glasgow are shown on the map by small circles: the triangle to the N.W. represents the position of Ben Ledi.

It will be observed that the isatmids, after passing the S.W., begin to open out and become much wider in a W.S.W. direction. This is probably an error due, as already stated, to the imperfect conditions under which the haze has to be estimated, making the W.S.W. winds appear clearer than they really are. The supposition that most of the isatmid lines are too wide for W.S.W. winds is supported by the fact that the isatmid for 2° keeps close to Falkirk for W.S.W. winds; the reason for this line keeping close being, that when the air is damp it is thick, and its haze is estimated in a short length of it. The estimate is thus made in the impure air itself, and the purer air in the distance has in this case no disturbing influence.

It seems probable that if allowance were made for the conditions under which the estimates had to be made, the other lines would also keep close to Falkirk till after passing the W.S.W. direction, and then somewhat suddenly open out to the W. If this be so, then while the influence of Edinburgh and the towns surrounding it is shown by the crowding of the lines to the east of Falkirk, the influence of Glasgow and its neighbourhood is only imperfectly shown by the incurving of the lines to the south-west. From this it will be seen that the isatmid lines show the effects of the unequal distribution of population in even the thickly populated parts.

The conclusions arrived at in this paper may be summed up as follows:—Accepting the two following conclusions, arrived at in previous communications on atmospheric dust, namely, that when the wind blows from populated areas the number of dust particles is always very great, and that for a given humidity the thickness of a haze is in proportion to the number of dust particles present, observations were made to test to what extent the air from populated areas was hazed, compared with that blowing over thinly populated districts.

Falkirk is so situated that the winds from the W., N.W. and N.

come to it but little polluted, whilst from all other directions it comes polluted by its passage over more or less densely populated districts.

By comparing the haze on days when the air had the same humidity, the relative transparency of the winds from different directions has been obtained,—that is, the relative transparencies of the air from polluted and from unpolluted areas.

Winds from the W., N.W. and N. are more than 6 times clearer than the southerly winds when the air is damp, and more than 9 times clearer when the air is dry enough to give 3° or more of wet bulb depression.

The air near Falkirk is about 10 times more hazed when the wind is E., S.E., S., and S.W., than it would be if there were no inhabitants in the country.

The transparency of the air increases with its dryness, becoming about 3·7 times clearer when the web bulb depression is 8° than when it is 2°. That is, the clearness of the air is inversely proportional to its relative humidity; or, put another way, if the air is 4 times drier, it is about 4 times clearer.

The isatmid lines for Falkirk for the different directions of wind, at different humidities, show that the density of the haze in our atmosphere is proportional to the density of the population in the direction from which the wind blows; the isatmid lines for all humidities being closer to the place of observation the denser the population in the direction from which the wind blows.

TABLE No. I.

*Showing the Mean Limit of Visibility in Miles of the Air at Falkirk for the Different Directions of Winds at Different Degrees of Dryness.*

| Direction of Wind. | 2° Number of Observations. | 2° Limit of Visibility. | 3° Number of Observations. | 3° Limit of Visibility. | 4° Number of Observations. | 4° Limit of Visibility. | 5° Number of Observations. | 5° Limit of Visibility. | 6° Number of Observations. | 6° Limit of Visibility. | 7° Number of Observations. | 7° Limit of Visibility. | 8° Number of Observations. | 8° Limit of Visibility. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| N. | ... | ... | ... | ... | 1 | 80 | 2 | 160 | ... | ... | 1 | 200 | ... | ... |
| N.N.E. | ... | ... | 2 | 50 | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... |
| N.E. | 3 | 32 | ... | ... | 5 | 33 | 1 | 35 | 1 | 25 | ... | ... | ... | ... |
| E.N.E. | ... | ... | ... | ... | 3 | 33 | ... | ... | ... | ... | ... | ... | ... | ... |
| E. | 3 | 10 | 4 | 5 | 5 | 18 | 2 | 18 | 1 | 15 | 2 | 14 | 4 | 22 |
| E.S.E. | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... |
| S.E. | ... | ... | ... | ... | 3 | 14 | 2 | 22 | ... | ... | ... | ... | ... | ... |
| S.S.E. | ... | ... | ... | ... | 3 | 12 | ... | ... | 1 | 12 | ... | ... | 2 | 15 |
| S. | 3 | 9 | 1 | 15 | 4 | 15 | 4 | 16 | 2 | 30 | 3 | 18 | 3 | 32 |
| S.S.W. | 4 | 6 | 2 | 11 | 8 | 11 | 3 | 13 | 3 | 13 | ... | ... | 2 | 45 |
| S.W. | 8 | 7 | 10 | 13 | 9 | 15 | 7 | 14 | 4 | 12 | 3 | 25 | 4 | 17 |
| W.S.W. | 1 | 4 | 2 | 25 | 1 | 25 | ... | ... | ... | ... | 1 | 50 | ... | ... |
| W. | 6 | 50 | 10 | 116 | 8 | 115 | 5 | 95 | 7 | 146 | 3 | 130 | 5 | 172 |
| W.N.W. | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | 1 | 150 |
| N.W. | ... | ... | 2 | 85 | 1 | 200 | 4 | 140 | 2 | 250 | 1 | 250 | 1 | 250 |
| N.N.W. | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... | ... |

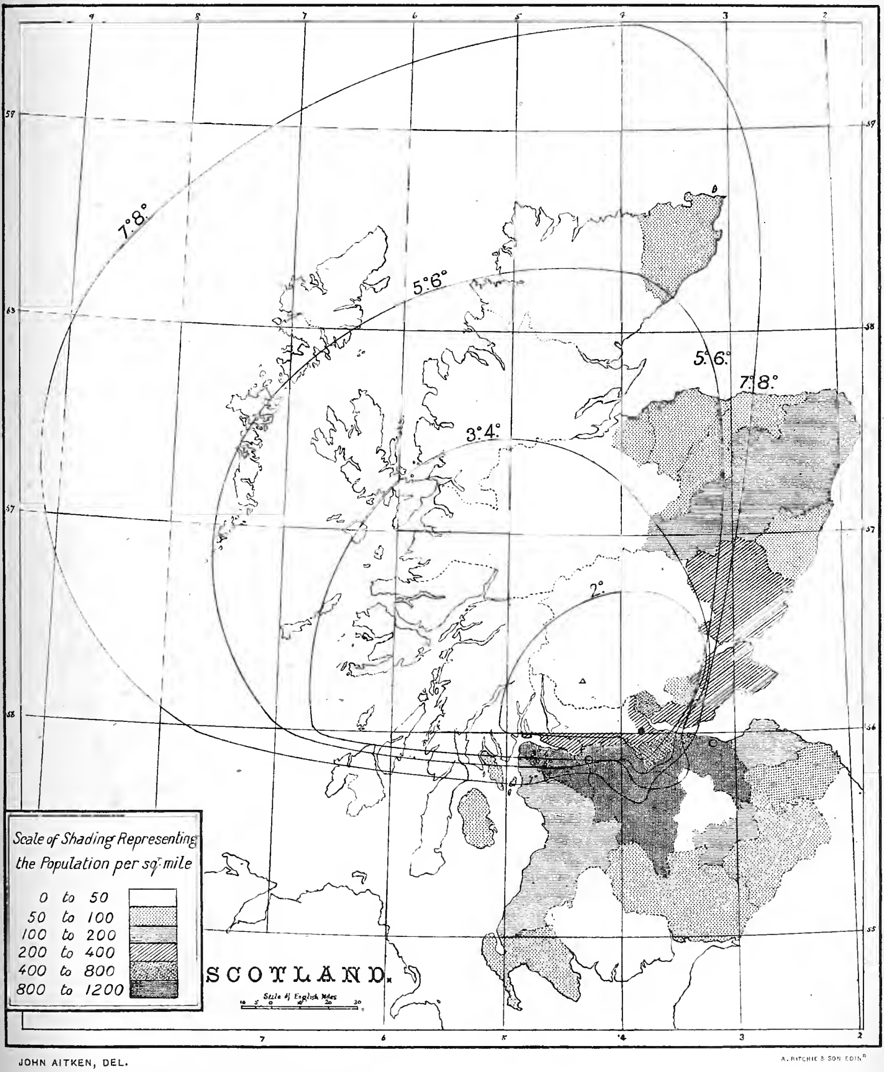
TABLE No. II.

*Showing the Mean Limit of Visibility in Miles of all the Observations when the Wind was Easterly and Southerly, and of the Winds when Westerly and Northerly, at different Wet Bulb Depressions.*

| Direction of Winds. | Limits of Visibility for Different Depressions. | | | | | | |
|---|---|---|---|---|---|---|---|
| | 2° | 3° | 4° | 5° | 6° | 7° | 8° |
| E., E.S.E., S.E., S.S.E., S., S.S.W, S.W., . | 8 | 11 | 14 | 18·5 | 16·4 | 19 | 26 |
| W., N.W., N., . . | 50 | 100 | 132 | 132 | 198 | 193 | 191 |

# MR. JOHN AITKEN ON THE HAZING EFFECTS OF ATMOSPHERIC DUST.

DIAGRAM REPRESENTING THE ISATMID LINES FOR FALKIRK FOR WINDS FROM THE DIFFERENT DIRECTIONS AND AT DIFFERENT DEGREES OF HUMIDITY.




JOHN AITKEN, DEL.

A. RITCHIE & SON EDIN^R

## Table No. III.

*Showing the Lowest and Highest Limits of Visibility observed at Falkirk for Winds from the Principal Directions and at Different Degrees of Dryness.*

| Direction of Wind. | Extreme Limits of Visibility in Miles for Different Wet Bulb Depressions. | | | | | | |
|---|---|---|---|---|---|---|---|
| | 2° | 3° | 4° | 5° | 6° | 7° | 8° |
| N. | ... | ... | ... | 120 to 200 | | | |
| N.E. | 25 to 40 | ... | 25 to 50 | 35 ,, 50 | | | |
| E. | 4 ,, 15 | 5 to 8 | 12 ,, 25 | 6 ,, 50 | ... | 12 to 15 | 12 to 50 |
| S.E. | ... | ... | 12 ,, 17 | 15 ,, 30 | | | |
| S. | 8 ,, 10 | ... | 12 ,, 20 | 12 ,, 25 | 30 to 30 | 12 ,, 30 | 15 ,, 50 |
| S.W. | 2 ,, 12 | 5 ,, 30 | 5 ,, 20 | 6 ,, 25 | 8 ,, 15 | 12 ,, 50 | 12 ,, 60 |
| W. | 20 ,, 60 | 30 ,, 250 | 75 ,, 200 | 50 ,, 200 | 50 ,, 250 | 120 ,, 150 | 60 ,, 250 |
| N.W. | ... | 70 ,, 100 | ... | 50 ,, 250 | 250 ,, 250 | | |

## Table No. IV.

*Showing the Mean Limit of Visibility of the Air in Miles at Falkirk for Winds from Different Directions, and for each Increase of 2 Degrees in the Wet Bulb Depression.*

| Direction of Wind. | 2° | | 3° and 4° | | 5° and 6° | | 7° and 8° | |
|---|---|---|---|---|---|---|---|---|
| | Number of Observations. | Limit of Visibility. | Number of Observations. | Limit of Visibility. | Number of Observations. | Limit of Visibility. | Number of Observations. | Limit of Visibility. |
| N. | ... | ... | 1 | 80 | 2 | 160 | 1 | 250 |
| N.N.E. | ... | ... | 2 | 50 | | | | |
| N.E. | 3 | 32 | 5 | 33 | 2 | 30 | | |
| E.N.E. | ... | ... | 3 | 33 | | | | |
| E. | 3 | 10 | 9 | 12 | 3 | 17 | 6 | 19 |
| E.S.E. | | | | | | | | |
| S.E. | ... | ... | 3 | 14 | 2 | 22 | | |
| S.S.E. | ... | ... | 3 | 12 | 1 | 12 | 2 | 15 |
| S. | 3 | 9 | 5 | 15 | 6 | 21 | 6 | 25 |
| S.S.W. | 4 | 6 | 10 | 11 | 6 | 13 | 2 | 45 |
| S.W. | 8 | 7 | 19 | 14 | 11 | 13 | 7 | 21 |
| W.S.W. | 1 | 4 | 3 | 25 | 2 | 33 | 1 | 50 |
| W. | 6 | 50 | 18 | 116 | 12 | 133 | 8 | 156 |
| W.N.W. | ... | ... | ... | ... | ... | ... | 1 | 150 |
| N.W. | ... | ... | 3 | 123 | 6 | 175 | 2 | 250 |

## Breath Figures. By John Aitken, Esq., F.R.S.

(Read May 1, 1893.)

These well-known figures are generally produced by placing a coin on one side of a piece of glass, and on the other side opposite it another coin or small plate of metal. The coin and the plate are then strongly electrified—the one positively, the other negatively. When the coin is afterwards removed, and the surface of the glass on which it rested is breathed on, there is developed an image of the coin, showing many of the details of the engraving. The image is produced by the condensed moisture being deposited in different-sized patches at different places. This is easily seen by examining the image by means of a microscope, using a low power, and illuminating the image by means of an ordinary mirror, with a large black spot fixed in the centre. At the parts where the image is bright, it will be seen that the moisture is deposited in very small detached patches of nearly equal size. The surface looks as if it were covered with a layer of small plano-convex lenses of quick curvature, with their edges close to each other. Where the image is darker the lenses are larger and flatter. As the amount of vapour deposited is about equal at all parts, the smaller the lenses the quicker will be their curvature; and it is the light scattered by the great number of small reflecting surfaces which causes one part of the image to be brighter than the other.

It seemed possible that in some cases these breath figures might depend in some way on the dust or other impurities on the surface of the glass, either deposited on or in some way altered by the electrical conditions to which it was subjected. If so, then it appeared to me that possibly heat might produce similar results by the molecular bombardment to which the surface of the cold glass would be exposed by the gases heated by the coin. When tried, heat was found to give breath figures similar to those produced by electrification.

The effect of the very fine dust from a flame when deposited on the glass was also tested; and as this experiment illustrates the manner in which the bright and dark parts are produced, which give the appearance of shading to the images, it will be described first. Clean a glass plate. Small pieces of mirror are the best for these experiments, as the images shine out more brightly on mirrors than on ordinary glass. The mirror should be polished till it shows an equal surface all over when breathed on. It may be remarked here that the ordinary process of testing the cleanness of a glass surface by breathing on it is simply a practical application of breath figures. Having got the surface free from figures, it is in a condition for receiving impressions. If we pass over this clean surface the point of a blow-pipe flame, using a very small jet, and passing it over the glass with sufficient quickness to prevent the sudden heating breaking it; and if we now breathe on the glass after it is cold, we shall find the track of the flame clearly marked. While most of the surface looks white by the light reflected by the deposited moisture, the track of the flame is quite black; not a ray of light is scattered by it. It looks as if there were no moisture condensed on that part of the plate, as it seems unchanged; but if it be closely examined by means of a lens, it will be seen to be quite wet. But the water is so evenly distributed, that it forms a thin film, in which, with proper lighting and the aid of a lens, a display of interference colours may be seen as the film dries and thins away.

Another way of studying the change produced on the surface of the glass by the action of the flame is to take the mirror, as above described, after a line has been drawn over it with the blow-pipe jet, and when cold let a drop of water fall on any part of it where it showed white when breathed on. Now tilt the plate to make the drop flow, and note the resistance to its flow, and how it draws itself up in the rear, leaving the plate dry. When, however, the moving drop comes to the part acted on by the flame, all resistance to flow ceases, and the drop rapidly spreads itself over the whole track, and shows a decided disinclination to leave it. This experiment shows why one part of the plate when breathed on is white while the other part is black. Over one part the condensed vapour draws itself into little lenticular patches, while over the other it spreads itself in a thin film, having no power to scatter light.

To produce breath figures by means of heat, similar to those produced by electricity, all that is necessary is to heat the coin in place of electrifying it. A little piece of mirror is cleaned and a coin placed on its surface. The coin is then heated and left to cool, after which it is removed from the glass. If the mirror be now breathed on, an image of the coin becomes visible. The coin may be heated by placing on it a piece of metal heated to the necessary temperature, but it has been found more convenient to heat it by means of a gas blow-pipe flame. A blow-pipe, with the air jet in the centre of the tube supplying the gas, and mounted so as to be capable of being pointed vertically downwards, does very well for the purpose, as by means of it the coin can be heated without the hot gases from the flame touching the glass. A temperature of 212° Fahr. gives results, but a higher temperature is better.

If we take a coin in the condition it is in while in circulation, and without cleaning it place it on the mirror and heat it, we shall get an image of the coin, which shows quite distinctly *without breathing on it.* The impurities on the surface of the metal are driven off by the heat, and form a whitish-looking deposit on the glass, sufficient to give a perfectly distinct image, showing most of the details of the engraving. The coin should therefore, before being used for producing breath figures, be heated to a higher temperature than it will be exposed to afterwards, and great care must be taken that the surface of the coin we are going to use is not touched afterwards, as it is found that if we clean the coin by heat, and afterwards finger it, it will again on being heated give an image visible without being breathed on.

The breath figures produced by heat are sometimes very distinct, every detail in the coin being reproduced with sufficient clearness to admit of the use of a magnifying lens to examine the details. In other cases, for reasons difficult to explain, the images are not so distinct; but, from the extreme minuteness of the alterations produced on the surface of the glass on which these figures depend, this is only what might have been expected.

Though the deposit of dust from a blow-pipe flame causes the condensed vapour to form a uniform film, yet this action of the surface of the glass, or rather of the impurities on its surface, may be destroyed, and the surface without being cleaned may be made to give the ordinary light-scattering form of condensation. If, for instance, the plate be highly heated, its surface will afterwards show some degree of whiteness when breathed on; and it has been found that breath figures can be produced rather better on a surface of this kind than on clean glass. If the glass, after being cleaned, be subjected to the action of the blow-pipe flame, the jet being made to travel quickly over it to prevent breaking, and then cooled, the coin placed on it, and then heated, breath figures will be produced, sometimes having great distinctness. The black parts being much blacker than when clean glass is used, sometimes these images on flame-treated surfaces do not require to be breathed on to be visible. The hot coin seems in some way to alter the dusty impurities on the surface of the glass, and causes them at certain parts to reflect more light than at others, so that the image is visible without being breathed on, but it is further developed when breathed on. These experiments show that dust does play a part in some kinds of breath figures.

Sometimes the impressions adhere very firmly to the glass, and are difficult to remove; but generally their removal may be effected by breathing on the plate, and by hard and frequent rubbing whilst it is wet. Water or alcohol may also be used with good results, but perhaps the most effectual means of destroying them is to pass over the surface of the glass either a blow-pipe or a Bunsen flame.

## On the Reproductive Organs of Noctua pronuba. By A. B. Griffiths, Ph.D., F.R.S. (Edin.), &c. (With a Plate).

(Read June 6, 1892.)

The Insecta are diœcious Invertebrates, and the rudiments of the sexual organs exist in the larvæ, but are chiefly developed during the pupal stage. These organs present such wide and manifold variations, that even those of individuals belonging to the same genus are so widely different in structure, &c., that it is only by a systematic study of each species that the student of nature is capable of arriving at correct conclusions concerning the reproductive organs of any particular species.

In the present paper the author gives an account of the reproductive organs of *Noctua pronuba* (Linnæus); as there has been no account given of these organs in this species, or even in this genus. The abdomen of both sexes consists of nine segments or somites, and in a state of rest the posterior two are retracted within the seventh segment; the intersegmental membranes in each case being long enough to admit of a telescopic action.

### The Male Organ and its Appendages.

The dorsal portion of the ninth segment of the male is produced backwards into a slightly curved hook (fig. 6, *a*). This hook covers the anal aperture, and in some Noctuidæ, as well as other Lepidoptera, is greatly developed. The ventral portion of the same segment forms a broad trough-shaped process (fig. 6, *b*), with upturned lateral edges, the penis (figs. 6 and 7, *c*) lying in the bottom of the trough so formed, and fastened to each side of the same segment are the claspers (fig. 6, *dd'*), which are narrow triangular pieces, each having a slightly incurved apex. There are two protrusible sac-like organs (fig. 6, *cc'*), which are also attached to the ninth segment, at the base of the claspers. These organs have delicate membraneous walls covered with long hair-like scales, which give each of them the appearance of a brush. Each organ may be protruded or retracted at will. The internal lining of these organs is an epithelium which secretes a fluid, for these organs are protruded by being filled with this fluid. This odoriferous fluid * finds its way outwards by the "hairs" which thereby become excretory ducts. It is probable that these appendages are "scent organs;" at all events there can be little doubt that they are organs for sexual excitation. They are entirely absent in the female, and, consequently, these organs act as an allurement to the female.

The male organ of reproduction consists of a large testis, two vasa deferentia, in each of which a tubular gland opens, a ductus ejaculatorius, and a penis. The kidney-shaped testis (fig. 6, *f*), which is pigmented, † is composed of two glands which have fused together during the metamorphosis. These glands are distinct in the larva, and lie close under the dorsal vessel in the fifth abdominal segment.‡

The vasa deferentia (fig. 6, *g*) lead from the posterior face of the testis. After a few convolutions they dilate into two curiously-shaped chambers, from which proceed two long capillary prostates (fig. 6, *h*), which ultimately unite with the mucous glands (fig. 6, *i*), into the basal portion of which the vasa deferentia seem to open. The vasa deferentia ultimately unite into a long and convoluted ductus ejaculatorius (fig. 6, *k*), which contracts slightly near its end, but again dilates into a muscular portion, which opens into the penis.

The penis is a slender, chitinous tube § whose tip projects between the claspers and below the anal aperture; and, as already stated, it lies in the trough formed by the ventral arch of the ninth segment. It is protruded by a muscle on either side which is attached to the same segment. From the terminal portion of the penis project two bifid hooks (fig. 8), which bear on the inner side several spines or teeth. These hooks appear capable, to a certain extent, of pro-

* Smelling of vanilla.

† This pigment appears to be one of the lipochromes.

‡ In certain Lepidoptera, the distinct condition of the testes is retained in the *imagines*.

§ The supporting substance in this organ is chitin, as was proved by its insolubility in solutions of sodium hydroxide, and its solubility in strong hydrochloric acid. After prolonged boiling with HCl, followed by evaporation, minute crystals of glucosamine hydrochloride were produced.

trusion and retraction; but the hooked penis in *Noctua*, although serving to retain the female, appears to be so constructed as to be broken in coition, consequently, that act cannot be repeated.

The semen appears to be transferred to the copulatory organ by a strong inflection of the posterior end of the abdomen, and it is then discharged into the vagina of the female.

The spermatozoa (fig. 9) are filiform, with rod-shaped heads and long flagella.

### The Female Organ.

The ovaries (fig. 1, *a*) consist of eight tubes, four of which lie coiled on each side of the body. Their slender tips end in suspensory ligments, all eight of which unite together immediately beneath the dorsal vessel. At the basal ends the ovaries of each side of the body unite into a uterine chamber (fig. 1, *b*), the short oviducts from which unite into a single oviduct, which passes through the eighth and ninth segments, and opens beneath the lateral flaps of the latter beneath the anal aperture.

Two colleterial glands, which have the function of secreting the egg-shell and the cement by which the moth fixes the ova in place when laid, open into the oviduct. The anterior colleterial gland (fig. 1, *c*) is single; while the posterior (fig. 1, *c'*) is a pair of glands with a single duct, or rather with ducts which ultimately unite into a single one. Both of these glands consist of long, convoluted, cœcal tubes, with oval-shaped dilatations near the base; and these are followed by another dilatation.

At the base of the eighth segment, on the ventral side, opens the vagina (fig. 1, *d*), the opening of which is distinct from that of the oviduct. It may be remarked, *en passant*, that in most Lepidoptera the vagina and the oviduct open by a common duct. The vagina of *Noctua* is a long canal with a smooth, stout cuticula, which passes into a large pyriform copulatory sac (fig. 1, *e*). The walls of this sac are very thick, and consist of a powerful muscular layer (fig. 4, *a*), within which is an epithelium layer (fig. 4, *b*), which gives rises to a thick cuticula (fig. 4, *c*). This cuticula lies in heavy folds (fig. 2), which have a general longitudinal direction, but with various curves and anastomosing branches. It is covered with little processes (cuticular spines), which are thicker near the base of the sac (fig. 3 illustrates these processes under high power). Near the base of the interior of the copulatory sac, a transverse triangular valve is suspended from above, which is also covered with the same kind of processes.

Near the base of the vagina, a slender sperm-duct (fig. 1, *f*) passes from the vagina into the oviduct, through which the spermatozoa pass from the copulatory sac, where they are discharged into the oviduct in which the ova are fecundated as they pass outwards. The sperm-duct does not expand into a spermatheca, as is the case in many insects. As already stated, the ova (fig. 5) are fecundated during their passage along the oviduct, and are then covered with the viscous secretion of the colleterial glands.*

It may be stated that the ova of *Noctua* reach maturity at the close of the pupal state, and, consequently, they are ready for fecundation and oviposition as soon as the pupal skin has been cast.† In some Lepidoptera (as well as other Insecta), there are two glandular organs, situated near the opening of the vagina, which secrete an odorous substance that excites copulation, or, in other words, that attracts the males. These glands appear to be absent in the female *Noctua pronuba;* in fact, the so-called "scent organs" are a distinctive feature of the male.

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* The viscous secretion of these glands hardens into a substance which gives the characteristic reactions of chitin.

† In certain insects (*e.g.*, *Libellula*) the ova do not reach maturity until a later period.

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## EXPLANATION OF PLATE.

Fig. 1. FEMALE REPRODUCTIVE ORGAN. *a* = base of four ovarian tubes containing ova ; only the terminal portion of the remaining four tubes is shown in the figure. *b* = uterine chamber. *cc′* = colleterial glands, the long tubes of which are not shown in the figure, having been cut off. *d* = vagina. *e* = copulatory sac. *f* = sperm-duct. *g* = copulatory vestibule. × 9.

Fig. 2. Cuticula from copulatory sac. × 52 (Zeiss's A, and 2 oc.).

Fig, 3. Small portion of same. × 320 (Zeiss's DD, and 3 oc.).

Fig. 4. Transverse section through one of the folds of the copulatory sac. *a* = muscular layer. *b* = epithelium layer. *c* = cuticula. × 1070 (Zeiss's J, and 4 oc.).

Fig. 5. Ova. *a* × 3 (about). *b* × 570 (Zeiss's J, and 2 oc.).

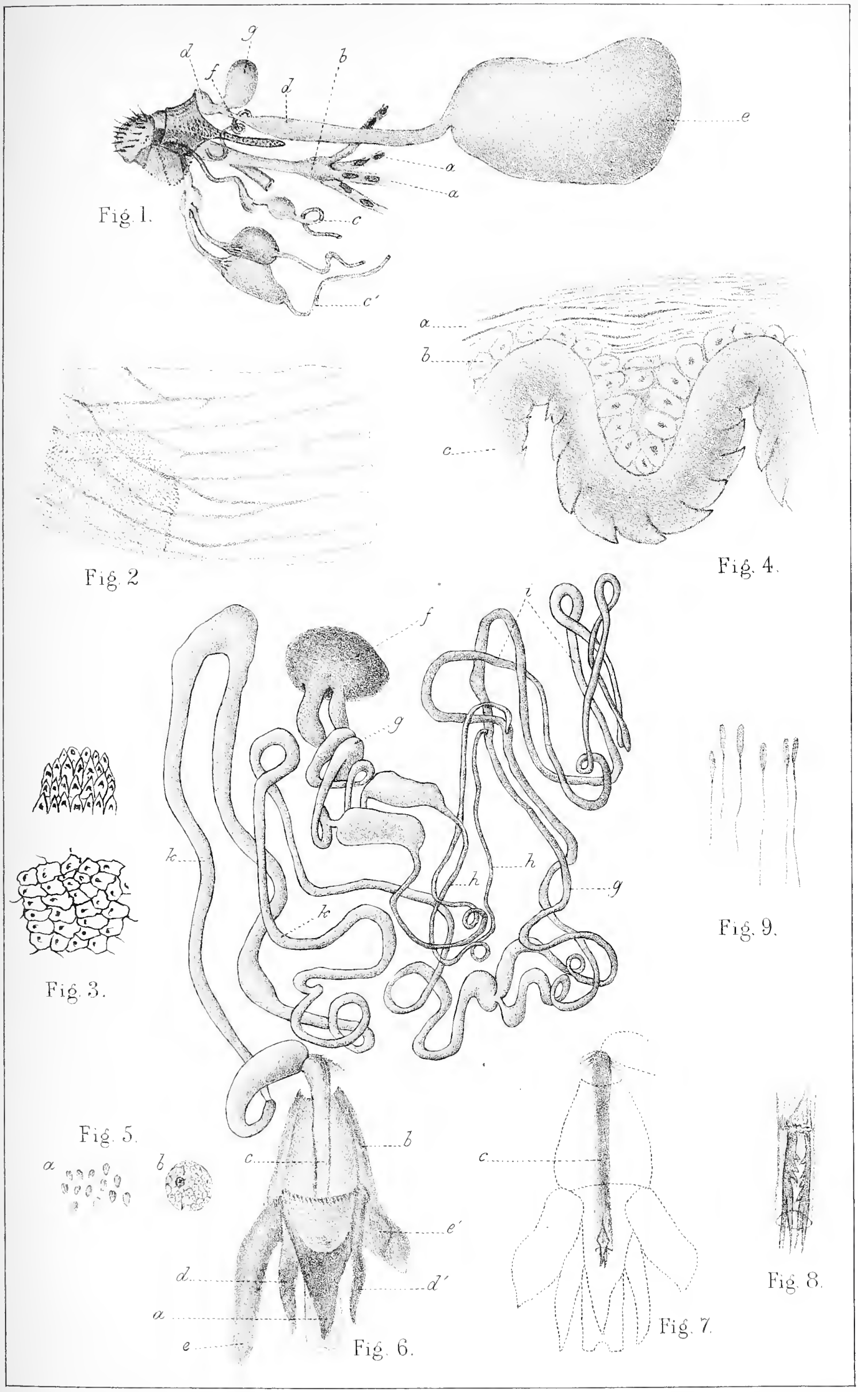
Fig. 6. MALE REPRODUCTIVE ORGAN (from above). *a* = hook of ninth somite. *b* = ventral process of ninth somite. *c* = penis. *dd′* = claspers. *ee′* = scent organs with hair-like scales removed. *f* = testis. *g* = vasa deferentia. *h* = prostates. *i* = mucous glands. *k* = ductus ejaculatorius. × 9.

Fig. 7. Penis (from below). × 9.

Fig. 8. Terminal portion of penis. × 20.

Fig. 9. Spermatozoa. × 1070 (Zeiss's J, and 4 oc.).

Dr. GRIFFITHS ON NOCTUA PRONUBA.




A. B. Griffiths, del.

McFarlane & Erskine, Lith.rs Edinr.

## Data on the Phenomena of Colour-Blindness, chiefly derived from Foreign Sources. *Compiled and arranged by* Dr William Pole, F.R.S., F.R.S.E.

(Read January 16, 1893.)

In the course of an investigation into the "Present State of Knowledge and Opinion in regard to Colour-blindness,"* it was found necessary to search for many data illustrative of various matters connected with the phenomena of the defect. These were obtained from a great variety of sources, chiefly from foreign publications not easily accessible; and as it was highly desirable to bring them forward in evidence, showing their bearing on difficult and controverted points, the plan has been adopted of collecting in this place the most important extracts, and of arranging them in a convenient manner for reference. With this view they have been in some cases translated and quoted entire; or, in others, a summary or abstract has been given of the principal matter they contain, distinguishing the various authors under each head by letters of reference.

The following is a classified list of the subjects and authors quoted:—

*On Dichromatism produced by Disease.*

| Letter of Reference. | | Letter of Reference. | |
|---|---|---|---|
| A. | Schelske. | C. | Von Kries. |
| B. | Nuël. | D. | Uhthoff. |

*On One-Eyed Colour-Blindness.*

| | | | |
|---|---|---|---|
| E. | Becker. | H. | Von Kries. |
| F. | Hippel. | I. | Hering |
| G. | Holmgren. | J. | Hess. |

*On Dichromatism in the Normal Retina.*

| | | | |
|---|---|---|---|
| K. | Schelske. | M. | Von Kries. |
| L. | Fick. | N. | Hess. |

* Published in the quarto *Transactions* of this Society, vol. xxxvii.

*On Variations in Dichromic Vision.*

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P. Ophthalmological Society.
Q. Donders.
R. Stilling.
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*Miscellaneous.*

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AE. Clerk-Maxwell: On the Palliation of Colour-blindness.
AF. Letters on the Law of Heredity.
AG. Pole's Notes on the Appearance of the Spectrum.

At the end will be found a list of the works and papers from which the extracts have been taken, and of others which have been consulted for the purpose of the inquiry.

A.

*Schelske's Case of Dichromic Vision by Disease,* 1865.

This case was tested by Maxwell's revolving discs, giving the following equations:—

33° yellow + 327° black = red.
200° blue + 160° yellow = grey.
150° blue + 210° yellow = bright green.

It was found that the patient saw a mixture of much red with a little blue, as identical with grey, and also that a certain blue-green was not to be distinguished from grey.

B.

*Nuël's Remarks on his Case of Dichromic Vision by Disease,* 1878.

The history of our *scotome centrale* appears to throw a bright light on the question, What are the colours really seen by the Daltonians?

The confusions of colour which our patient makes at the level of his *scotome* are, in fact, the same confusions as are made by persons born colour-blind; but while with the latter there has often been uncertainty, our patient was absolutely positive in his answers; not showing the least hesitation or the least contradiction.

The prevailing opinion, dictated by Helmholtz, is that the colour-blind, who persist in designating yellow by the name of yellow, and blue by the name of blue, see in reality green and violet instead of these two colours. The authority of Helmholtz in this matter is so great that there has been an almost general reception of his view, which has been imagined in order to satisfy the Young-Helmholtz theory of the three specific energies of the retina.

Well! here is a patient presenting us with a retina which, in a part of the area, is Daltonian, while, in the other part, it acts normally. He has at every moment a point of comparison at hand, and the names which he gives to the colour sensations ought to correspond with those used by the normal eyed.

The result is that the colour-blind see really *yellow* and *blue*, and not green and violet as the theory would require us to believe.

## C.

### *Von Kries's Account of Dichromatism by Disease*, 1882.

During the progress of the disease the distinction of green becomes difficult, and at last impossible; green then becomes yellow or grey. The next stage is that red cannot be distinguished from yellow; only blue can be rightly so distinguished, all else is yellow or grey. Violet becomes dark blue, and red is usually denoted as black. At last even blue becomes invisible, and total colour-blindness sets in. Whenever red fails, green fails also.

## D.

### *Uhthoff on Dichromatism by Disease*, 1887.

He found four cases in each of which there was a total loss of the red and green sensations over the whole field of the eyesight, the vision becoming perfectly dichromic. One of these cases was carefully examined by Dr König with spectral colours, in the same way as the colour-blind (whom König was then elaborately examining). The patient was found to have a neutral point like them, and the intensity curves of his two colours were defined. The blue colour did not materially differ from that of others, but the yellow colour formed an intermediate between that of the "red-blind" and that of the "green-blind," both as to the position of its maximum and its general form.

The same author in speaking of cases of atrophy of the optic nerve (Græfe, vol. xxvi.) says, "Wherever red fails, green fails also."

### E.

### *Becker's Case of One-Eyed Colour-Blindness*, 1879.

This was a curious case. A little girl only three years old had remarked to her mother that objects in the room looked a different colour according as she lay in bed on her right or her left side. This led to an examination, when it was found that the same objects appeared of different colours to her right or her left eye. The abnormal eye proved to be almost totally blind to colour. This was the first case clearly made out where the two eyes had different colour vision.

### F.

### *Notes by Von Hippel on his Case of One-Eyed Dichromic Vision*, 1880–81.

There was no shortening of the spectrum at either end. When shown the full extent of the spectrum, the patient described the extreme left-hand end rightly as red; the yellow and green he called "yellowish and greenish," all the remainder blue. But when the colours were divided into small strips, so that he could not see what part of the spectrum they belonged to, then from the extremest red, through yellow to the green-blue he described all as yellow, and from thence to the violet end all as blue. The single nuances of red, yellow, and green appeared, indeed, differing in brightness, but all of the same quality of colour, and the same with blue and violet. The brightest spot in the spectrum for the right (dichromic) eye was to the right of the sodium line, about in the middle between it and the green calcium line; but for the left eye (normal) it was left of the sodium line. With incandescent metals the red lithium line, the green thallium line, and the yellow sodium line appeared perfectly alike in colour, but the two first slightly less bright than the last. It was, the author says, interesting to see how, after making mistakes with the defective eye, he always explained and corrected them by the aid of the normal one.

As to the hue of the two colours, Hippel says:—"I have given the patient the yellow sodium, the blue indium, and the cæsium lines alternately with the normal and the abnormal eye. I have always obtained the most definite assurance that the lines appear absolutely alike in hue, only to the blind eye a little weaker in luminosity. The two eyes behave in an analogous manner in regard to white; one sees white exactly as the other does, only the dichromic one a little paler."

The neutral line was between *b* and F, but was difficult to define, it changed with the changing illumination; with increasing brightness it went nearer to F; with diminishing brightness nearer to *b*. The brightest parts in each colour were also difficult; but after many trials they were for the yellow close beyond D, for the blue between F and G, rather nearer the latter.

The author remarked that the defect must be congenital, because the nature of the colour-disturbance agreed perfectly with the congenital defect, and there was absolutely no symptom of anything wrong with the other eye. He said in conclusion:—"I see no possibility of bringing these facts into accord with the Young-Helmholtz hypothesis, without doing the greatest violence to it, and giving up its fundamental principles; whereas they come easily within the scope of the Hering theory, to which they present no difficulty in any direction."

## G.

*Extracts from Professor Holmgren's Paper given to the Royal Society of London,* 1881.

A combination of a normal and an abnormal eye with the same brain is not impossible. . . . . . I have been fortunate enough to examine two such cases; one a case of one-sided red-blindness.

A one-sided colour-blind person has, through his normal eye, a perfectly clear conception of normal-eyed people's different colours, and can tell his conception, by the aid of his other eye, for others normal sighted. His definitions are thus, in opposition to persons colour-blind in both eyes, perfectly reliable. I have in every instance let the person in question point out an objective colour with his normal eye for every one of his conceptions with his abnormal one. Indirectly we find in this way which qualities of perception are wanting in the abnormal eye, in comparison with the normal one.

The two principal colours in the spectrum for the red-blind are, as to their fundamental tone, *yellow* and *blue*. The yellow commences a little later, reckoned from the end, than the red of the normal-eyed (about Fraunhofer's line C), and stretches over the rest of the red, orange, yellow, yellowish-green, and ends in the blue-green (between Fraunhofer's lines *b* and F, nearer to the latter), where a narrow neutral colourless belt forms the limit against the other principal colour, *blue*, which stretches through the remaining part of the spectrum corresponding with our cyan-blue, indigo, and violet.

The tone of the red-blind person's first fundamental colour is not perfectly golden-yellow, but seems, for the normal eye, to have a shade of greenish-yellow, perhaps best defined as citron-yellow in the lighter, and as olive-green in the darker shades. His other fundamental colour does not seem to be purely cyan-blue or indigo, but is rather a blue with a perceptible shade of violet. It might be called indigo-violet.

## H.

### *Remarks by Von Kries on Hippel's One-Eyed Case.*

We find that the spectrum is shortened at the red end, and that consequently a relative want of sensitiveness for red light exists. We may also assume that the matches will be such as are attributed to the typical red-blind, *i.e.*, that a very luminous red will be matched with a faint blue-green. If we draw from this fact the conclusion that the red component has fallen out, and in consequence only the green and the blue (or violet) components are excited by white light, then the Young-Helmholtz theory leads to the conclusion that the white of the abnormal eye will be the complementary colour to red, *i.e.*, green-blue. But this is contradicted by experiment, for the fact is that the colour-blind eye sees white as white, or precisely the same as the normal eye. . . . . . The warm colour of the red-blind is not green, but yellow with a slight green tinge.

Blue and yellow shadows were correctly described with the right eye just as with the left; what to the left eye were red and green appeared to the right eye as colourless or grey. Other tests were applied in which, though the left eye saw them as normal, the right eye saw the usual dichromic appearances, giving a positive confirmation to the ordinary ideas as to the two main colours being yellow and blue.

## I.

### *Remarks by Professor Hering on a One-Eyed Case of Dichromic Vision,* 1890.

(1) All the colours used appeared to the affected eye less saturated, *i.e.*, much whiter or greyer than to the healthy eye.

(2) Yellow and blue appeared yellow and blue; and suffered no remarkable change of their hue (*ihres Tones*), but were seen much less saturated.

(3) Green and red, which were near the primitive red and green (*Urroth und Urgrün*), and which were not highly saturated, appeared colourless to the affected eye.

(4) The intermediate colours, viz., spectral red, orange, yellow-green, and violet not too much saturated, lost for the affected eye their red or green, and therefore appeared yellow or blue, but very pale and greyish.

(5) White, grey, and black were seen by the affected eye as by the healthy one, *i.e.*, perfectly colourless.

These results lead to the conclusion that the power of the affected eye to see yellow and blue in comparison with the power of the colourless sensation, is materially lessened, but the power of seeing red and green has very nearly disappeared. Thus the affected eye is nearly red-green blind, and has a much weakened sensitiveness to yellow and blue.

It is superfluous to show specially that the facts described cannot be reconciled with the Young-Helmholtz theory, for it follows obviously from them that spectral red, green, and blue or violet are not the variable elements of the colour sense; but rather, on the one hand, the pure yellow and blue, and, on the other hand, the pure red and green; *i.e.*, those two pairs of colours which I have designated as the fundamental or the original pairs of colours.

## J.

### *Hess's Case of Dichromatism in One-Half of an Eye*, 1890.

Two months previously the vision of the patient was perfectly normal, but at the time of his examination the nasal half of his left eye had become colour-blind, while the temporal half remained normal. Tests were applied, both with pigments and spectral colours, and it was found that the affected half presented all the symptoms of nearly complete dichromic vision. The sensitiveness to red and green was almost entirely lost, but that to the yellow and blue remained, although weaker. The white was slightly lessened in luminosity, but not changed in colour.

## K.

### *Schelske's Experiments on the Dichromic Zone of the Retina*, 1863.

This writer tested the vision of the middle zone of the retina with Maxwell's revolving discs, and obtained the following equations:—

45 yellow + 55 blue = 18 white + 82 black.
24 yellow + 76 black = red.
41 yellow + 59 blue = dark green.
52 yellow + 48 blue = light green.
10 yellow + 43 black + 47 white = light green.

The author fully admits the analogy of the case with congenital dichromic vision. To illustrate this, however, I have plotted the above equations in a diagram, which may be compared with my own and others of the same kind, as given in the *Philosophical Magazine* for July 1892. The author only gives the general names, and not the exact nuances of the test colours used; but the general correspondence will be quite evident.

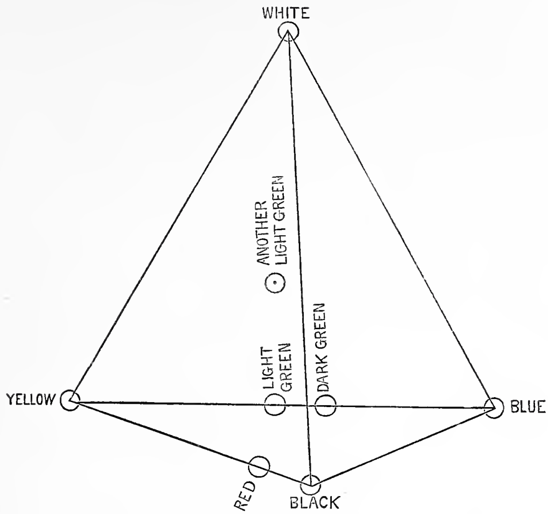



Diagram of the Colour Vision of the Middle Zone of the Normal Retina, as observed by Schelske.

## L.

### *Investigations by Professor Fick on the Dichromic Zone of the Retina*, 1874.

The multiplicity of colour-sensations is confined to a small central part of the retina round the pole. Beyond this extends a zone which behaves like the retina of the red-blind eye; and farther away, nearer the equator, is another zone, where all qualitative sense of colour is lost. The following is the nature of the impressions given by the same ray in the three zones respectively :—

| Polar Zone, Normal. | Middle Zone, Dichromic. | External Zone. |
|---|---|---|
| Red. | Yellow or brown, according to luminosity. | Grey or black. |
| Yellow and green. | Bright yellow. | White. |
| Blue. | Paler blue. | Grey. |
| Violet. | Blue. | Grey. |
| Blue-green. | White or grey. | Grey. |

In a later publication (1879) he connected these appearances more explicitly with congenital colour-blindness. He said:—"In a considerable fraction of all mankind (more than one-twentieth) the polar zone of the retina is also wanting in the normal capability of distinguishing the varieties of colour. Such individuals are called 'colour-blind.'" It is probable that this state simply consists in the fact that the property of the retina, which in the normal eye exists in the middle zone, is here present also in the *fovea centralis.* Total colour-blindness will further ensue if the whole retina takes the properties of the extreme outer zone of the normal eye.

## M.

### *Remarks by Von Kries on the Dichromic Zone of the Retina*, 1882.

This author discusses the whole subject very fully; and on the main points he agrees with other investigators. He fully substantiates the existence of an intermediate annular dichromic zone, where only yellow and blue are visible, the red, orange, and yellow-green being transformed into the former, and the blue-green and violet into the latter.

The limits of the zones are somewhat irregular and variable; the vision of green in the smallest extending some 30° or 40° from the centre; that of red is a little more; then comes the blue and yellow ring extending to about 65°, and the peripheral or colourless space filling up the remainder. The divisions are not sharp, but the changes run gradually into each other.

The author also discusses the theory, and points out the difficulties with the Young-Helmholtz hypothesis.

## N.

### *Hess's Observations on the Variable Colour Vision of the Normal Retina.*

It had been remarked in previous investigations that the transitions between the different colour zones of the normal retina were not sudden, but gradual, and this author determined to watch more carefully than formerly the whole of the colour changes, and the effect of various exciting rays, in every position of their passage from the centre to the circumference. The trials were made first with pigments, then with revolving discs, and finally with spectral colours.

The results fully confirmed what was known before, and gave additional weight to the important fact of the *permanence* of the two complementary pairs of colours, namely, definite varieties of yellow and blue, and of purple-red and blue-green. Any other colour, the moment it passes out of the central area of vision, will begin to change, and will ultimately turn into one of the four colours named. Moreover, in the final extinction of these colours, as they approach the colourless peripheral zone, the red and green pair first disappear, the yellow and blue remaining much longer. White light remains unchanged over the whole retina.

Thus the two complementary pairs stand out as naturally and strongly distinguished from all other hues, the yellow and blue pair having a special preference. And it is difficult to avoid the inference that these properties must have an important bearing on the explanation of colour-blindness.

## O.

### *Cohn's Examination in the Government Schools,* 1877.

At the beginning of his work he did not know what theory would best apply, and chose tests which would throw light on the matter. Out of 3490 young persons examined, 100 were found with defective vision, and were then examined very fully with spectral and other tests of various kinds. Eighty showed unmistakeably they had no correct normal sensations of either red or green, for the most part placing the brightest part of their spectrum in or near the yellow. The following are opinions extracted from his summary :—

No system can be deduced from the comparisons which these persons made with the wool-tests, they were too varied. If really the red and the green blindness were distinct from each other, the patients would have made certain matches which they did not make.

Every red-blind person is also green-blind; every green-blind person is also red-blind. If mistakes with green are ever so small, mistakes with red are also discovered.

It is only by the careful investigation of every case with all the indicated methods, and by an exact analysis of every case, that this result is arrived at. If any one distinguishes red-blindness from green-blindness, it is only because he has not examined into the case with sufficient accuracy.

### P.

### *Extract from the Report of the Ophthalmological Society of Great Britain on Colour-Blindness,* 1880.

Defective vision of red and green is the form exhibited by every one of our 617 pronounced cases. A red or green blind person fails in appreciation of both colours. An attempt has been made to separate persons found by the wool-test to be colour-blind into the "red-blind" and the "green-blind." But though, according to this classification, a large number, perhaps two-thirds of the whole, stand clearly under one or the other head, there are many who appear equally related to both.

The distinction between red and green blind is apt to mislead.

### Q.

### *Investigations by Professor Donders of the Variations in Dichromic Vision.*

These investigations are so extensive, and so much scattered about in the author's works, that it is difficult to give a succinct account of them in a reasonable space. He gives tables and diagrams of experiments made, and he established the existence of wide variations, tending to collect in two extreme forms which, for convenience, he called "red-blind" and "green-blind," but without attaching to these terms their literal signification.

The green-blind had, in the part of the spectrum from D to the red end, pretty nearly the normal length of spectrum and strength of colour-sensation, differing very little from each other, and these formed the larger portion of the whole.

The red-blind were deficient in the strength of colour, with frequently a spectrum shortened at the red end; they were less uniform, and intermediates between the extremes were not wanting.

The maximum luminosity for the green-blind was near D, but for the red-blind was nearer to E. The neutral point for the green-blind was on the average about 502·8 ; for a typical red-blind about 494·8.

## R.

### *Professor Stilling on Colour-Blind Vision*, 1883.

This author points out that the chief element of variation is the variable sensitiveness of the patients to the influence of the long waved rays, and he divides them into three classes :—

(1) Those who have the normal sensibility for these rays, *i.e.*, those who, though they do not see the normal colour, receive in the pseudo-colour about the normal degree of colour-giving power. [This is my case.]

(2) Those whose sensitiveness in this way is less than the normal, so that although the whole of the rays impress them, it is with less than the normal power.

(3) Those who have a shortened spectrum at the red end ; and to whom, therefore, some portions of the long-waved rays are absolutely powerless.

These grades, however, run into each other.

The author also refers to variations in the effect of the green-producing rays, which he says may sometimes be less than in normal vision. But he does not seem to notice the connexion between the red and the green sensitiveness, in contrary directions, shown in the intensity curves, and in my observations. (See T in these data.)

## S.

### *Later Views of Professor Holmgren on Variations of Dichromic Vision*, 1881 *and* 1884.

Although Professor Holmgren was a supporter of Young's colour theory, he did not agree in its original application to the explanation of colour-blindness. And when I was in communication with him in 1881 I learnt that, in consequence, although he used the terms red-blindness and green-blindness, he did not consider they really applied to the subjective sensations of dichromic patients, inasmuch as he believed that the two kinds always went together. But he continued to use the terms as a matter of convenience, in order to give an idea of the different objective behaviour, in sorting and matching external colours, which is manifested by those having different varieties of the affection.

In a letter to me, dated 28th March 1881, he says:—"We seldom find two colour-blind persons of the same class who agree in all particulars. But that is the case in all Nature. There are no two plants or animals perfectly alike, but they may, nevertheless, have certain common characters which may justify us in a systematic separation of them. I was the first to succeed in showing that the "red-blind" cannot subjectively see either red or green, and so may it well be with the green-blind. Thus the names do not refer to the subjective sensations of these persons."

"I hold that neither of the theories now popular is perfectly right. But I adhere to the practical objective distinctions between colour-blind persons, who, in actual fact, treat certain kinds of light in different ways. And I am of opinion that these two classes of persons, who may be distinguished by my wool-test, may well be described as red-blind and green-blind. For so long as we possess no means of looking into the subjective sensations of other people, we shall do well to base our classification on objective signs. I believe further, that I have already found that the exact hues of the colours of the colour-blind are not always the same."

In a later letter, dated February 1882, he says:—"When I have asserted that the results of observation are consistent with Young's theory, I only mean with the principles of the theory, but not with the form in which the theory is usually applied."

A year or two after this, in Professor Holmgren's lecture, given at the Congress at Copenhagen in 1884, he gave full particulars of the remarkable cases he had examined of dichromic vision in one eye, and he exhibited diagrams of the appearance of the spectrum to them. It happened that these cases included patients of both varieties, and he described the colours that they saw. He said:—"The two principal colours seen by the 'red-blind' patient were yellow, inclining towards green; blue, inclining to violet. While for the green-blind patient they were orange-red and cyanide-blue" (*i.e.*, blue, inclining to green).

The positions of the neutral points were for the "red-blind" near F, and for the "green-blind" nearer *b*.

## T.

### *Explanations and Remarks on my own Observations of the Variations in Dichromic Vision,* 1859 *and* 1892.

The observations are described in the *Philosophical Magazine* for July 1892, and they show considerable variations of the precise kind met with in practice.

It will be seen that the more important variations lie in two particulars only, viz., in the colour intensity (or, as I call it, the "chromic strength") of the impressions produced by the rays corresponding to the normal red and green. There is little doubt that the two extreme cases fairly represent the two classes into which dichromic vision has been erroneously divided. I myself form one extreme, having been pronounced "green-blind." Mr Parry forms the other extreme, and would certainly have been called "red-blind." The others are intermediate between these.

I had no means of testing the cases with spectral colours; but, thanks to the elegance and the accuracy of Clerk Maxwell's contrivance, the facts come out clearly and consistently.

Let us first look at the impressions of the red rays. Carmine, the strongest specimen of the normal red colour, required to match it for me 10 parts of yellow mixed with 90 of black; for Mr Parry it required only 3 parts of yellow with 97 of black; showing that the colour impression made by the carmine on me was $3\frac{1}{3}$ times as strong as on Mr Parry. Probably this colour may have corresponded to somewhere in the spectrum between B and C, so that, as it was scarcely visible to Mr Parry, he must have had a shortened spectrum.

Vermilion required $23\frac{1}{2}$ parts of yellow for me; 8 for Mr Parry, or about 3 to 1. Orange gave a proportion of 5 to $3\frac{1}{2}$.

Now let us turn to the *green* element, of which there are two specimens, a very dark one, brunswick-green, and a lighter one, emerald-green. With these colours the comparative strengths are reversed. For me brunswick-green required only $9\frac{1}{2}$ parts of yellow, for Mr Parry it required 15. Emerald-green required for me 23 parts, for Mr Parry 43 parts. Hence, the "chromic strength" of the green colour may be said in round numbers to be about twice as great on Mr Parry as on me.

These facts are corroborated by another test of a different character, viz., the quantity of blue required for neutralisation. For me vermilion required 0·37 proportion of blue, for Mr Parry 0·11 were sufficient; while emerald-green required for me 0·37, for Mr Parry 0.66.

These two cases are the extremes, and if we look at the other cases we find them irregularly *intermediate.* For example, the chromic strength varies in the five columns thus :—

| | | | | | |
|---|---|---|---|---|---|
| For vermilion, . . | 235 | 130 | 85 | 80 | 80 |
| For emerald-green, . | 23 | 33 | 28 | 42 | 43 |

and, similarly for other data—strong presumptive proof against *grouping.*

I may call attention also to what I think is a new feature in these data, viz., the distinction between the effects of the *saturation* and the *luminosity* on the colour impression.

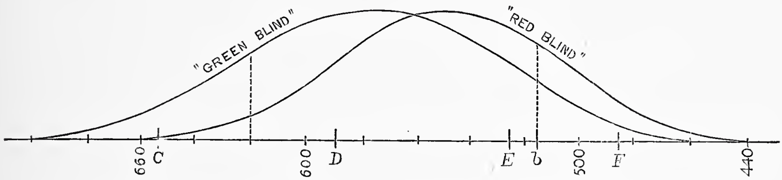
The impressions given by the red rays are nearly all perfectly *saturated ;* whereas those given by the green rays are only saturated to about 80 per cent. This accords perfectly with the spectral explanation given in my figure 5, and page 111. Then, again, the saturation remains constant, or nearly so, for each colour through the whole series ; the variation being in the *luminosity* only. This points to a distinct *cause* of the variability.

The luminosity of the neutral mixture of red and blue is very small, and varies to some extent with the power of the red impression. That of the neutral blue-green is much greater, and nearly constant, owing to the large mixture of white with the green rays.

It is a striking feature, in contrast with the red and green variations, that the relations of the four fundamental colours—yellow, blue, white, and black—with each other, as shown in Equation I., are essentially the same in all the cases, showing, I think, a general uniformity of the whole of the experiments. I was not aware, at that early date, of the extreme precautions now thought necessary in regard to the colour of the daylight, &c., but I think the series shows that the care taken was sufficient to get fairly good results.

But although the fundamental colours agree so well, there is clearly a tendency to variation generally, which is usually slight, but becomes strongly marked in the red and green. This, combined with the absence of grouping, may be said to be the essence of the results obtained.

It may now be shown how well these results correspond with the careful determinations of the intensity curves obtained a quarter of a century later. The following diagram represents the intensity curves of the yellow colour-sensation as determined in 1886 by König and Dieterici for two dichromic patients; the left-hand curve for one called "green-blind," the right-hand curve for




one called "red-blind." For facility of comparison the ordinates have been so arranged as to make the maximum intensity the same[1] in both curves.

Suppose we now take a point at 620 between C and D, where red rays fall, we find that the intensity of the colour impression, measured by the ordinate of the curve, will be about four times as great for the "green-blind" as for the "red-blind." But if we take a point, say at *b*, where green rays fall, we find that the colour impression on the "red-blind" is nearly 50 per cent. greater than on the "green-blind." At 500 it is about double.

It is curious, too, how these curves confirm the peculiar feature in my experiments, that the difference in the impressions of the two classes is so much greater with the red rays than with the green ones.

We can also now explain the tests applied to distinguish the two classes by their matches (see *Transactions*, page 459). To the person with the left-hand curve, a given red ray will appear yellower than to him with the right-hand curve, while a given

[1] In the original diagrams (copied in Helmholtz, p. 367) the ordinates for the maximum intensities vary, but this is done for a special object not wanted here. The ordinates do not in any case represent real intensities, but only proportional ones for each curve separately.

green ray will appear less yellow. If, therefore, he has to find a green colour which will match to him a given red, he must obviously choose a much "yellower green" than would match for the right-hand curve patient. And this is exactly what Helmholtz gives as the test for "green-blindness."

We arrive, then, at the conclusion, as shown by the comparison of the experiments and the curves, that the whole of the phenomena characterising the variations of dichromic vision may be attributed to the simple fact of a slight variation of the position of the yellow sensation curve along the line of the wave-lengths, so that this sensation may be excited to a given extent, in two different persons, by waves slightly differing in length: the maximum difference, equal to some 30 or 35 millionths of a millimetre, will then constitute the difference between the extremes, formerly called "red-blind" and "green-blind."

There seems nothing in this to suggest different "groups" in classification, or any important fundamental difference in the cause.

## V.

### *Lord Rayleigh's Experiments on Variations in Colour Vision,* 1881.

The author says;—"The trials revealed an interesting peculiarity of colour vision quite distinct from colour-blindness. The red and green mixture, which to my eyes and to those of most people matches perfectly the homogeneous yellow of the line D, appeared to my three brothers-in-law hopelessly too red, 'almost as red as red sealing-wax.' In order to suit their eyes, the proportion of red had to be greatly diminished, until to normal sight the colour was a fair green with scarcely any approach to yellow at all. These peculiarities were quite unexpected. . . . . .

"I have obtained matches between simple and compound yellow from 23 male observers; of these 16 agree with myself within the limits of observation. The remaining seven include my three brothers-in-law and two others. The vision of the other two observers differs from mine in the opposite direction. . . . . . Among seven female observers whom I have tried, there is not one whose vision differs sensibly from my own."

Thus, out of 31 persons observed—

24 may be called normal.

5 differ considerably from them, in having either much increased susceptibility for the red or much diminished susceptibility for green.

2 have abnormal susceptibility in the opposite direction.

The author adds :—"Mr Balfour requires only half as much red as myself in order to turn a given amount of green into yellow; on the other hand, Mr Hart requires much *more* red than I do, in the ratio of about 2·6 to 1."

## W.

### *Experiments by Von Kries and Frey*, 1881.

These observers, after referring to the work of Maxwell and Müller, undertook experiments partly to obtain the results of a series of mixtures of spectral colours, partly to learn something more exact about the individual differences of observers, to which the phenomena of colour-blindness had given a special interest.

Their first series of experiments was the direct determination of complementary colours by two different observers. Their next was the mixing of red and green to make yellow; and the next for mixtures of green and violet. The observations showed that the results varied with the two observers from 20 to 50 per cent.

They thought that the most obvious explanation was the assumption of a difference in excitability of the sensitive fibres, or absorption by the yellow pigment of the retina.

## X.

### *Experiments by Donders on Variations of Normal Vision.*

He tested 60 persons, all, generally speaking, normal eyed, by mixing a red at the lithium line (670) with a green at the thallium line (535) in such quantities as to form the yellow of the sodium line (590).

Fifty-six persons ranged from 31 green + 69 red to 23 green + 77 red.

The other four required much more green, *i.e.*, from 67·9 green + 32·1 red to 57·7 green + 42·3 red.

The mean results were, that while with the majority 1 part of green required 2·65 parts of red, with the minority 1 part of green required only 0·82 part of red.

He devoted much attention to a consideration of the possible cause of this remarkable variation, but failed to attribute it to any other source than a real variation of the susceptibility to the colour impressions.

## Y.

### *Experiments on the same subject by König and Dieterici.*

These experimenters examined their own vision. They found for one observer 1 green balanced by 2·66 red; for the other 1 green balanced by 3·25 red.

But they found two other persons who required respectively 1 green with 0·96 red and 1 green with 0·71 red.

The two investigators plotted their colour intensity curves. They agreed tolerably well as to their red impressions, but they differed so much in their green sensations as to require separate curves in the diagram; these were much the same shape and area, but differed in position by about 5 millionths of a millimetre in the wave-length scale. They also plotted a third green curve which was found to vary still more remarkably from the two others, arguing very different sensations throughout this part of the spectrum.

These results were adopted and remarked on by Helmholtz in the new edition of his work.

## Z.

### *Hering on Individual Variations in the Colour Sense,* 1885.

The object of this elaborate essay is to account for the variations of the red and green impressions in dichromic vision; and he begins by calling attention to the analogy of similar variations in normal vision.

He mentions remarkable cases of this that had come under his own observation. He found that with two observers making a match for his fundamental red with colour cards, one required 261° red + 99° blue, the other 347° red + 13° blue; the ratio $\frac{R}{B}$ being in one case = 2·6, in the other = 26·7.

In another example, with five persons, combining a spectral red of *w.l.* 660 with a spectral blue of 447, the proportion $\frac{R}{B}$ in the five cases was

$$= 1{\cdot}15,\ 3{\cdot}00,\ 4{\cdot}71,\ 5{\cdot}66,\ 7{\cdot}00.$$

To make his fundamental green with a yellow-green and blue, the proportion $\frac{G}{B}$ for two individuals was 5 and 16. In the relations between yellow and blue no difference in hue was observed.

He then gives explanations and arguments at much length, tending to show that the cause of all these variations, as well as of the variations in dichromic vision, is probably to be found in varying absorptions by colouring of the macula, and to a certain extent of the lens also. He also considers that the stronger absorption will be accompanied by greater sensitiveness to colour generally.

This being so, his explanation would be as follows:—

A dichromic patient A. has strong sensitiveness and strong absorption. He will be powerfully impressed by the long-waved rays (red); but his strong absorption diminishes the colour of the shorter rays (green). This is the "green-blind."

Another dichromic patient has weaker sensitiveness and weaker absorption. The former diminishes the force of the long-waved rays (red), but the less absorption gives him a stronger sense of the short ones (green). This is the "red-blind."

Helmholtz objects to this:—(1) That the colouring of the lens does not exist to any appreciable extent in healthy eyes; and (2) that the colouring of the macula only acts on a limited portion of the visual field and only on certain colours. But he admits that further investigation on this point is desirable.

## AA.

### *Fick's Explanation of the Dichromic Colours,* 1874 *and* 1879.

Referring to the dichromic zone of the normal retina, he remarked that the ordinary application of the Young-Helmholtz theory would not account for the colours it showed. He pointed out in particular that two of the fundamental colour-sensations would not make white, but that their equilibrium must be deeply saturated hues of green, blue-green, green-blue, or blue, which is in contradiction with fact.

He preferred a more probable assumption. He chose, on reasonable grounds, as Maxwell did, blue for one of the three fundamental colour-sensations, and considered that the other two, the red and the green, might coincide and form yellow. He further showed that by a further coincidence of the blue sensation the outer zone, of light and shade only, might be explained; and he gave a diagram to show how this might occur.

In 1879 he published a more elaborate essay on vision generally, in which this explanation was further insisted on.

## AB.

### *Krenchel's Remarks on Theory*, 1880.

The author says :—"It is perhaps possible to bring the several physiological and pathological facts in unison with the three fundamental colours of Young and Helmholtz,—even the apparently opposing facts seem lately to have been explained, according to this elastic theory, by a suitable alteration of the 'excitement curves.' But it is very suspicious that the facts are explained with equal ease by Hering's theory of two pairs of fundamental colours. And even this similar suitability of two different hypotheses seems to point to the conclusion that their correspondence with fact does not say much in proof of their value.

A hypothesis is only allowed when it is necessary, *i.e.*, when some difficulty in regard to the comprehension of the phenomena is removed by it. If the phenomena explain themselves without the hypothesis, the latter seems to me unnecessary and unallowable.

We only know two phenomena of the whole process of colour perception—the first cause, *i.e.*, the coloured rays, and the final effect of them, *i.e.*, the colour impressions. All the physiological process which lies between them is unknown. Now there is nothing about fundamental colours in either of these phenomena. The spectral colours are all equally unmixed; the several colour impressions are in themselves not recognisable as mixed. Why therefore is it necessary to assume that these simple final effects can only arise from the simple first causes by the intervention of mixing operations? Where are the difficulties of comprehension which are removed by the assumption of fundamental colours?

The author then states the various reasons which have prompted the assumption of fundamental colours, and shows that the facts are capable of explanation without them.

He does not deny that it is possible to produce all colours (mixed with white) by mixtures of three, of which one is arbitrarily chosen; but this is no reason to justify giving them the name of fundamental colours. He admits the ingenuity and elegance of the theories, but believes that many of the industrious labours, especially in regard to colour-blindness, which are carried on in all directions, would produce more results of permanent value for science if they were not based so dogmatically on particular theories.

Some of the most accurate investigations on colour-blindness show cases that cannot properly be grouped under either of the hypotheses. They go the easiest with Hering's, which gives the idea (so well corresponding with the facts) of pairs of colours. But it appears very possible that the affection for which "red-green blindness" is a very suitable name, if taken in a sufficiently wide interpretation, really embraces a whole series of different kinds, including the red and the green blindness of Helmholtz.

The author's object is not to set up a new theory, but to weaken the undue reliance on the old ones, so that the freedom of investigation may not on all sides be limited and cramped thereby.

## AC.

### *Investigations by Drs König and Dieterici,* 1883 *to* 1886.

Some of the most elaborate investigations we have on colour vision were made about this period by Dr König and Dr Dieterici, two pupils and collaborateurs of Professor von Helmholtz. The experiments appear, indeed, to have been made partly under his guidance; he presented accounts of them to the Berlin Academy; and he has incorporated many results of them in his own book. They are, therefore, eminently worthy of attention.

Dr König has been one of those who, although a stanch supporter of the Young-Helmholtz theory, has openly dissented from its original application to colour-blindness. In the inquiry of 1883, he explicitly stated that, according to recent discoveries, it had been proved that dichromatism could not be explained by the assumption that one of the fundamental colours was absent, and the other two were in action. And in 1886, in his communication to the British Association, he stated the same thing, admitting that the colours were yellow and blue. But, although holding this opinion, he made a most elaborate investigation to test the applicability of the rejected explanations. In this he was joined by Dr Dieterici, and the results were given in a paper presented to the Berlin Academy on "The Fundamental Colour-Sensations, and the Distribution of their Intensity in the Spectrum." The paper was of so much importance that it is desirable to give some account of it.

The authors began by stating that the first step necessary was to analyse the series of spectral colours by reducing them to a minimum number of "elementary sensations," the combinations of which, in different proportions, would give all the spectral hues. For this purpose they chose three—one near the red end, one near the violet end, and one in the green, distinguishing these elementary sensations as R, V, and G respectively. Then they had, by proper instrumental experiments, to find the proportions of each that would give the various hues, and finally to draw the "elementary sensation curves" representing the colour vision of different persons. All this had been done before by Clerk Maxwell, Donders, and Van der Weyde, but the new investigators did it with more care, and with a special view to further researches later on.

They tried in this way three kinds of vision—viz., total colour-blindness; dichromic vision; and normal vision. The dichromic interests us most here. They tested the so-called "green-blind" and "red-blind," and their results agreed tolerably well with those of Van der Weyde. The warm curve for both classes was nearly similar in form, but for the green-blind it was situated nearer the red end of the spectrum, its maximum being at 570, whereas the maximum for the red-blind was about 550 to 555. The neutral points were about 502 for the green and 492 for the red-blind, but were indistinct and difficult to decide on. The cold colours appeared to coincide, with a maximum at about 455. The curves of various kinds are copied in Helmholtz's work, pages 358 and 367.

The next thing was to endeavour to test the applicability to these facts of Young's theory of three fundamental sensations. But in order to do this they found it necessary to adopt a new and extended view of their nature.

The general plan had been to consider the three before-mentioned "elementary sensations" R, G, and V, taken from the spectrum, as the "fundamental" ones. But the authors had reason to think that the result would be better if they endeavoured to find other colours for the purpose, and to assume the spectral hues as all obtained by mixture from these. This was not an entirely new idea, as in 1859 both Clerk-Maxwell and myself had found that the red which was invisible to dichromic vision (and which, therefore, was considered as fundamental) was extra-spectral; and others had suspected the general mixed nature of the spectral colours.

The fundamental sensations had to be the same in number as the elementary ones, and connected with them by linear equations of the form

$$x = \frac{aR + bG + cV}{a + b + c}$$

The object König and Dieterici had in view, as they expressed it, was to ascertain whether, among the infinite number of possible fundamental colours, they could find three such that a person who could only see light and shade should have one of them; a dichromic person should have two of them; and a normal eye all three of them. They were obliged to give up the first condition, but after much intricate calculation they found colours that would comply with the conditions for dichromic and normal vision.

It must suffice to give the results of the painstaking labours in searching for these fundamentals. They were ultimately settled as follows:—

(1) A red, inclining more to purple than the extreme end of the spectrum.

(2) A green of wave-length about 505.

(3) A blue of wave-length about 470.

And it was found that "the two types of dichromic vision hitherto investigated might be considered to arise out of the normal trichromic system, in such a way that in one type the fundamental red sensation, and in the other type the fundamental green sensation, were wanting."

Diagrams illustrating these fundamentals are copied in Helmholtz, pages 340 and 370.

The authors remarked that the three sensations, thus found, corresponded with those which Hering, by a pure physiological analysis, had designated as "*Urroth*, *Urgrün*, and *Urblau*," and that the complement to the blue sensation was his *Urgelb* of *w.l.* 575.

They also pointed out the possible coincidence of the red and green curve, producing the dichromic yellow; they called this "the modern development of Thomas Young's theory of colour-blindness," and took credit for the proof it afforded that "the views of Thomas Young, slightly modified by modern experimental research, were perfectly correct; and that in this way modern science seemed to have breathed into his theory of colour vision a life of such vigour that it would flourish for ever!"

Their results were afterwards modified by Helmholtz; but, nevertheless, their labours were very valuable, and contributed materially to his later solution of the problem.

## AD.

### *Hillebrand on the Specific Luminosity of Colours.*

This is an elaborate essay on a complicated and difficult subject, viz., the connexion of luminosity and colour. After an introduction by Hering, alluding to some changes of his own views, the author, in a long chapter, discusses the nature of the subject, and the import of several terms used (Helligkeit, Lichtstärke, Intensität, Sättigung, &c.). He then, after mentioning the previous investigations by Fraunhofer, Vierordt, Brücke, Macé, and Nicati, explains his own views and objects. He considers that the total "Helligkeit" of any spectral colour is made up of two factors:—First the power which the colour possesses of exciting the sensation of white; and secondly, a "specific luminosity" of the colour itself, varying for different colours. He has found means of determining the former of these by experiment; and he has made approximate estimates of some of the latter.

It is curious that Helmholtz, in the new edition of his great work, although he does not allude to Hillebrand's researches, notices the duplex nature of the luminosity. He says, p. 440:—"For my part I have, throughout, the ideal impression that in heterochromic comparisons of luminosity, we have to do, not with comparison of one quantity, but with the joint action of two, luminosity and colour-glow, for which I am unable to form any simple sum, and which I am also unable scientifically to define."

## AE.

### *Professor Clerk-Maxwell on the Palliation of Colour-Blindness.*

There is a general impression that as red-green blindness is congenital, and as there is no means known of changing it to normal vision, the sufferers have no remedy. This is a mistake. As early as 1855 Professor Clerk-Maxwell pointed out, in his paper to the Royal Society of Edinburgh (page 287) that much relief in regard to the confusion of red with green could be given by coloured spectacles, one eye being furnished with a red and the other with a green glass. The effect of using these would be that, looking through the red glass, the reds would appear brighter and the greens darker, while the green glass would reverse these effects.

He was kind enough, at a later time, to send me such a pair; and on my asking "what was his charge for them," he wrote me the following admirable letter:—

Glenlair, Dalbeattie, N.B.,
*10th November* 1869.

My Dear Sir,—With respect to the spectacles my charge to you (and it is a strict one) is that you should put them on and look at some variegated object, such as a bright-coloured carpet or needlework, comparing the appearance of doubtful colours as seen by each eye. I mean colours which are doubtful to you, whether they are called by others red, green, or drab.

Of course it will be possible for you to find out by a process of reasoning whether any particular colour is red, green, or drab, by observing whether it appears much brighter, much darker, or nearly the same through the two glasses. You will then act like a chemist testing bodies apparently similar by different reagents.

But this is not all that I want you to do. I especially want to find out whether a person can, by using the spectacles repeatedly, always with the red glass over the same eye, acquire the power of applying the test without any conscious process of reasoning, just as we estimate distances by the eye. Of course this is a matter requiring time and trouble, and I have not persuaded any other colour-blind person to go through with it, and I only ask you to do so because you take an interest, not only in colour-blindness, but in other "sensational science," such as sound, and therefore are able to attend to your sensations in a direct manner better than most people who only use their sensations as a means of perceiving things.

With respect to the illustration I made use of concerning the perception of distance there is this difference. The distance of a visible object is a matter of great importance in every case, and every one has a clear idea of it. But the distinction of red and green is of small importance to you, and the necessity of it is not always pressing upon you, as in the case of distance. Besides, you can never acquire our notion of what is meant by it.

If I were to wear a pair of spectacles made of Nicol's prisms, and could turn them, or my head, easily round, I should get the power of perceiving the plane of polarisation of light, and it is possible that by constantly attending to the connection between the appearance of polished bodies and the plane of reflection of the light, I might come to perceive unconsciously the inclination of any polished surface to the line of sight, so as to have a much more vivid perception of the solidity, say of a jet ornament or a polished boot, than I now have.

In fact, we have a certain degree of this faculty, for the central spot of the retina has an imperfect power of analysing polarised light, and the thing seen goes by the name of Haidinger's brushes. When I look at a bit of blue sky, I generally see the sign of the brush, which tells me in what direction the sun is; but as the phenomenon is not a very obtrusive one, I am not surprised that many people never observe it, and that it has not become developed into a new power of perception.

If you should be able, by means of the spectacles or otherwise, to assist at the transformation of a scientific testing process into a new power of a so-called direct perception, you will have conferred a great favour, I do not say on me, but on the human mind; and if you do not succeed, the spectacles will be a monument of a defeated curiosity.

Yours truly,

T. CLERK-MAXWELL.

I have the spectacles still, and have used them frequently; and although I cannot say I have arrived at the full result contemplated by Professor Maxwell, I have abundantly proved the power they give of an instantaneous and most positive distinction between hues of red and green which appear alike, or nearly so, to the naked eye. If, for instance, I look at the three squares in fig. 3 of my paper in the quarto *Transactions* (which to my naked eye appear all alike), using the red glass, the red figure is much brightened and the green one much darkened; while with the green glass these effects are reversed, the middle figure being unaffected in both cases.

## AF.

### *Letters to Dr Pole on the Law of Heredity in Colour-Blindness.*

*5th May* 1880.

Dear Sir,—In an article of yours in the *Contemporary* on "Daltonism" you remark on the immunity of women from colour-blindness. The following family history seems to show that this is, in a sense, compensated by a peculiar tendency to transmit it to their sons:—

My maternal grandfather's maternal grandfather ($A$) was a Jerseyman. I know not whether he was a Daltonist or not, but something odd in the family seems suggested by the fact that though he had seven or eight grown-up sons, all died childless, leaving their four sisters, my great-grandmother and great-great aunts, the sole representatives of the family.

One of these ladies died unmarried; the eldest (a) married; the second (b) married my great-grandfather; the third (c) married.

(c) had, I believe, some sons, one of whom was colour-blind; but that line of $A$'s descendants then died out.

(a) had, as far as I know, only one son ($A_2$). He was so colour-blind that he nearly got into trouble by writing an official letter in two colours.

$A_2$ had two sons and two daughters. None of these were colour-blind.

The elder daughter of $A_2$ is unmarried. The younger ($a_2$) had a family of two sons and four daughters. The younger son, $A_3$, is excessively colour-blind; the rest of the family are free.

My great-grandmother (b) had three sons and two or three daughters, who married. The posterity of her daughters is numerous, and I have never heard of colour-blindness among them, or that they were colour-blind themselves.

The eldest brother of my grandfather was faintly colour-blind. He had many sons and daughters, first cousins of my mother, none of whom were colour-blind. I think none of the daughters have left children.

The eldest married son of my great-uncle has four sons, eight daughters, and a daughter's son, all free. The only other married son of my great-uncle left four daughters, who have most of them large families, but none of them are colour-blind. My grandfather's younger brother was free from colour-blindness, and so was his family, but it was small and has died out.

My grandfather, $B$, who was very colour-blind, had about sixteen children, none of whom were colour-blind. His sons, my uncles, have large families of sons and daughters, and several of them have grandchildren; but colour-blindness is unknown among them.

On the other hand, his five married daughters, my mother and four aunts, have the following families:—

*First Aunt* ($b_2$).—Two sons, a daughter, four or five sons' sons, and a son's daughter. $B_2$, the elder son, my first cousin, is colour-blind; his brother, sister, sons, daughter, and nephews are free.

*Second Aunt* ($b_3$).—Three sons and several daughters, and the children of a son and of some daughters. My aunt's eldest son, $B_3$, is colour-blind, the rest of her posterity free.

*Third Aunt* ($b_4$).—One son, $B_4$, excessively colour-blind.

*My Mother* ($b_5$).—Seven sons, three daughters, and a son's son and a son's daughter. My eldest brother, $B_5$, my third brother, $B_6$, and I, the sixth son, $B_7$, are colour-blind, the rest of my mother's posterity free. Though I was the youngest patient, my colour-blindness was the first noticed, when I was five years old; but my eldest brother is the worst case, and my third brother the mildest.

*Fourth Aunt* ($b_6$) has two sons and two daughters; the elder son, $B_8$, colour-blind, and the rest free.

The result is this, that the sons of the patient and their descendants are free. The daughters of the patient and their daughters and their descendants are also free. In fact, one son, or two female descents, break the spell. But where the daughter of a patient has sons, one of these at least is always a victim, and will transmit it to his daughters' sons.

I don't know whether this is a general rule, but I may suggest that the fact of the change of surname, which such a descent entails, would operate much to conceal such a canon of descent from observation. People by their "family" generally mean their agnatic relations, and none of my agnatic relations, except my brothers, have the defect.—Yours faithfully, $B_7$.

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16*th June* 1893.

DEAR SIR,—In the thirteen years since I wrote to you, the descendants of my colour-blind relations have largely increased. But I know of no case of colour-blindness among my nephews, nieces, first cousins once removed, second cousins once removed, or third cousins once removed. $B_2$'s daughter is lately married. It will be curious to see if she has a colour-blind son. I believe that a daughter of $B_3$'s married some time ago, but I don't know whether she has a family or not. I think these two are the only two directions in which colour-blind children can be immediately expected.

It is rather noticeable that the (a) line differs from the (b) line, in that the eldest son of $a_2$ was not colour-blind, whereas the eldest son of b was partly colour-blind, and the eldest sons of $b_2$, $b_3$, $b_4$, $b_5$, $b_6$ are all colour-blind. This seems to suggest that the *eldest* son of the colour-blind man's daughter is peculiarly liable to inherit the defect.

My brothers and I had noticed the law of transmission several years before 1880, but I neither remember the exact date nor to which of us it first occurred to do so. We had never read anything on the subject.—Yours faithfully, $B_7$.

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The above description being necessarily somewhat complicated, I have tried to express the course of descent of the malady, through the five generations, more clearly in the following diagram, where the *italics* distinguish the colour-blind descendants. W. P.

<table>
<tr><td>GENERATIONS. I.</td><td colspan="8">*Ancestor*, *A*<br>(Assumed to be Colour-blind).</td></tr>
<tr><td>II.</td><td>Daughter, a.</td><td colspan="6">Daughter, b.</td><td>Daughter c.</td></tr>
<tr><td>III.</td><td>*Son*, $A_2$.</td><td colspan="5">*Son*, *B*.</td><td>*Son*.</td><td>*Son*.</td></tr>
<tr><td>IV.</td><td>Daughter, $a_2$.</td><td>Daughter, $b_2$.</td><td>Daughter, $b_3$.</td><td>Daughter, $b_4$.</td><td>Daughter, $b_5$.</td><td>Daughter, $b_6$.</td><td></td><td></td></tr>
<tr><td>V. (Present.)</td><td>*Son*, $A_3$.</td><td>*Son*, $B_2$.</td><td>*Son* $B_3$.</td><td>*Son*, $B_4$.</td><td>*Son*, $B_5$,<br>*Son*, $B_6$.<br>*Son*, $B_7$.</td><td>*Son*, $B_8$.</td><td></td><td></td></tr>
</table>

## AG.

### Notes of my own Observations on the Appearance of the Spectrum, 1879.

*At Prof. Lockyer's Laboratory, South Kensington, 29th July* 1879.

Observed an air spectrum, made by an electric spark, which gave a discontinuous spectrum of a great many lines.

All the left hand ones appeared yellow; all the right hand ones appeared blue. The division between the colours was well marked by a broad line, which appeared to me white. [This, I was told, was a green double line of nitrogen, *w.l.* 500·5 and 500·2.] This had a narrower line on each side of it, pretty close; the left hand one appearing yellow, the right hand one blue.

*At the same place, 11th August* 1879.

Observed the spectrum of white light from the sky; no sun.

At the red end, Mr Miller and I could just distinguish line B, but nothing beyond.

The yellow colour, going towards the red end, begins to darken about half-way between D and C, and the darkening increases till the illumination disappears, just like the shading of a round column in an engraving, the yellow colour being always maintained.

Going from the maximum yellow towards the right hand, the colour begins to *pale* off shortly before E, and gets paler, more watery, up to the division of the colours. This is quite a different effect from the shading at the red end; it is clearly caused by the admixture of white light, and the consequent reduction of intensity of colour.

From near E to half-way between C and D the colour is very brilliant, and seems tolerably uniform.

The division of the yellow from the blue appears about half-way between *b* and F, or may be nearer *b*, say $b\frac{1}{3}$F; but it is indistinct and variable, flashing sometimes bluer, sometimes yellower.

From the division, going towards the blue end there is first the same paleness, the colour gradually augmenting to about G, where it seems full. Beyond this, uncertain; the colour seems full and deep, but somewhat dark; vision near end indistinct, flashing; cannot positively trace the shading. As to the extremity visible, Mr Miller can see just as far as the line *h*, 410·1. I cannot see the line, but I can see the blue colour nearly as far; boundary indistinct and changeable, flashing.

*Potassium Spectrum.*—I can see the extreme red line perfectly distinct; it appears a dark shade of yellow; cannot, after many trials, see the extreme violet line, but can see diffused colour nearly as far.

*Spectra* of *Na. Lith.* and *K.* combined. The three lines at red end very beautiful. *Na* (589·3) powerful, full yellow; *Li* (670·5) also powerful and full, but rather darker; *K* (768) also coloured, but much darker.

### *At Mr Hilger's Manufactory*, 15*th August* 1879.

Moderate-sized spectroscope; daylight, occasionally sunlight.

Red end and division confirm former notes.

Paid more attention to the blue end. From the division it begins pale and watery like the yellow part, increasing in saturation to about G. Then it becomes *darker*, but the light being feeble, I do not see the shading so characteristically as on the yellow end. It appears to die gradually off, losing light, till it disappears, and at the same time remaining full in colour, quite different from the light pale appearance near the division.

Mr Hilger saw the two H lines very distinctly, but to me they were quite invisible; all was in darkness where he put the pointer on them, and my colour did not extend so far. But I found by the scale that it went to about $\frac{19}{20}$ of the distance from G to H, *i.e.*, very near to H.

### *At Dr Huggins's Observatory, Tulse Hill*, 15*th October* 1879.

The object of this visit was to observe the spectrum, with the aid of Dr and Mrs Huggins, and to determine carefully its appearance to me. This was done with a fine spectroscope, using either solar or daylight illumination, or electric sparks with different metals.

I afterwards embodied the result in a coloured drawing, which I sent to Dr Huggins on the 18th October. (A chromo-lithograph imitation of this is given in the plate to my article in the *Transactions* of this Society, vol. xxxvii.)

The division between the colours was confirmed at about the nitrogen line, which appears a dull, colourless hue, a sort of grey, not brilliantly white.

At the *red end*, trying several lines, potassium, &c., I could see everything that Dr and Mrs H. saw. At the *violet end*, I saw the two H lines exhibited by calcium, and also when dark, but only faintly. Mrs H. could see farther at this end than Dr H., and he could see farther than I. On the whole, my length of spectrum does not differ materially at either end from that of normal eyes.

Dr Huggins showed me a copy of Fraunhofer's diagram of the *gradations of luminosity* in the spectrum. I made, at his suggestion, observations by putting a wedge of grey glass in front of the spectroscope, increasing the thickness till I shut out the light. These observations showed that the gradations of luminosity in the various parts of the spectrum correspond in my vision with the normal condition.

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## LIST OF SOME WORKS AND PAPERS

That are referred to in the foregoing Data, or have been examined with reference to Dr Pole's Paper on Colour-Blindness.

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"Græfe."—Von Græfe's *Archiv für Ophthalmologie*, Berlin.
"Du Bois-Reymond."—*Archiv für Anatomie und Physiologie* (Physiological Part). Leipzig.
"Pflüger."—*Archiv für Physiologie*, Bonn.

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## Preliminary Note on the Compressibility of Aqueous Solutions, in connection with Molecular Pressure. By Professor Tait.

(*Abstract.*)

(Read June 5, 1893.)

The experiments referred to in my paper of March 6th (*anté*, p. 65) have been completed, but the results are by no means so exact as I hoped to make them. There was great difficulty in procuring the small bore tubes for the piezometers, and thus I had to employ them without previous calibration, as the solutions to be experimented on had already been prepared, and their densities determined at definite temperatures. Delay might have led to evaporation. When I proceeded to the calibration, after completing a large series of experiments, I was greatly annoyed to find that the bores of many of the tubes were by no means uniform. This accounts for the fact that my experiments, though fairly concordant, are not sufficiently so to afford more than a very strong probability in favour of the general result of the inquiry. For this reason I have described my paper as a Preliminary Note.

The idea I sought to develop was of the following nature. I had found that the average compressibility of water, at any one temperature, could be well represented by the simple formula

$$\frac{A}{B+p},$$

where $p$ is the range of pressure through which the compressibility is measured; A and B being functions of temperature. But I also found that for aqueous solutions of common salt, of different strengths, and at the same temperature as the water, the formula was altered to

$$\frac{A}{B+s+p};$$

where A and B were as before, and $s$ was proportional to the weight of salt dissolved in 100 of water. In particular that, when 1 ton-weight per square inch (152·3 atmospheres) is the pressure unit, $s$ is nearly the weight of salt in 100 of water.

Theoretical speculations (given at some length in my *Report on some of the Physical Properties of Water*, Challenger Reports, Physics and Chemistry, vol. ii., 1888) led me to look on the B, and the $B+s$, of these formulæ as being connected with the molecular pressure in the liquid, and I developed one application of them, relating to the maximum density points of various solutions of common salt.

The present series of experiments was conducted precisely as were the earlier ones, but unfortunately many of the piezometers (of which a large number were required in order that several solutions should be operated on at the same time) were new, and (as I afterwards found) faulty. The selection of the salts was undertaken by Dr Crum Brown, and the solutions were made and the density determinations effected in his Laboratory by Mr A. F. Watson.

I give these at once, as they have intrinsic value altogether apart from my work and my hypothesis.

In the following table the letters S and W stand for the masses of salt, and of water, respectively. Mr Watson remarks that the error in the numbers of the first column, from which the second was calculated, does not exceed 1 in 1000. The error in the densities does not exceed unit in the fourth decimal place.

| $100\frac{S}{S+W}$ | $100\frac{S}{W}$ | Temp. C. | Sp. Gr. | Temp. C. | Sp. Gr. |
|---|---|---|---|---|---|
| Potassium Iodide— | | | | | |
| 14·538 | 17·011 | 5°·5 | 1·1197 | 13°·5 | 1·1179 |
| 9·302 | 10·256 | 5°·6 | 1·0737 | 12°·2 | 1·0727 |
| 4·313 | 4·507 | 5°·4 | 1·0329 | 12°·0 | 1·0323 |
| Potassium Ferrocyanide— | | | | | |
| 14·089 | 16·399 | 5°·5 | 1·0987 | 13°·5 | 1·0967 |
| 9·411 | 10·389 | 6°·3 | 1·0620 | 12°·1 | 1·0610 |
| 4·753 | 4·990 | 6°·0 | 1·0328 | 11°·4 | 1·0322 |
| Ammonium Sulphate— | | | | | |
| 15·938 | 18·960 | 6°·8 | 1·0954 | 11°·2 | 1·0944 |
| 9·232 | 10·171 | 6°·3 | 1·0559 | 12°·7 | 1·0547 |
| 5·301 | 5·597 | 5°·7 | 1·0326 | 12°·1 | 1·0317 |

Magnesium Sulphate—

| | | | | | |
|---|---|---|---|---|---|
| 13·836 | 16·058 | 6°·8 | 1·1489 | 11°·2 | 1·1479 |
| 9·508 | 10·507 | 5°·8 | 1·1005 | 13°·1 | 1·0990 |
| 5·869 | 6·235 | 5°·7 | 1·0614 | 12°·1 | 1·0602 |

Barium Chloride—

| | | | | | |
|---|---|---|---|---|---|
| 13·798 | 16·006 | 5°·8 | 1·1366 | 11°·2 | 1·1354 |
| 9·096 | 10·006 | 5°·8 | 1·0869 | 13°·1 | 1·0855 |
| 4·585 | 4·805 | 5°·6 | 1·0423 | 12°·2 | 1·0416 |

To these may be added the following, due to Dr Gibson, from my Challenger Report referred to.

| Sodium Chloride— | 0° C. | 6° C. | 12° C. |
|---|---|---|---|
| 17·6358 | 1·138467 | 1·136040 | 1·133565 |
| 13·3610 | 1·101300 | 1·099341 | 1·097244 |
| 8·8078 | 1·067589 | 1·066144 | 1·064485 |
| 3·8845 | 1·029664 | 1·028979 | 1·027935 |

Although I made at least two observations at each of the pressures 1, 2, and 3 tons, on each solution, in each of two piezometers, I publish in this Abstract nothing beyond some mean results at one temperature and for one pressure :—viz. 12° C. and 2 tons. These are fairly representative of the whole work. The columns of mercury used in calibration corresponded nearly with the parts of the tubes concerned in the measured compression at that pressure; and, on such lengths of tube, errors of measurement due to slight changes of temperature of the solution, &c., are comparatively insignificant.

The change of (unit) volume of water per ton at 12° C. and 2 tons is (by my former work)

$$\frac{0{\cdot}2474}{36+2} = 0{\cdot}00651\,.$$

If to the 36 in this expression be added the product of the quantity $s$ below given for any one salt, multiplied by the per-centage of the salt, we have the numbers in the column headed *Calc.* Those headed *Obs.* were obtained as stated above; and the agreement is on the whole satisfactory. The old determinations for common salt are included in the table, though they show rather less concordance than the others.

| $100\frac{S}{W}$ | $s$ | Obs. | Calc. |
|---|---|---|---|
| Sodium Chloride— | | | |
| 17·6 | 1·1 | 0·00428 | 0·00431 |
| 13·4 | | 472 | 470 |
| 8·8 | | 524 | 519 |
| 3·9 | | 594 | 585 |
| Magnesium Sulphate— | | | |
| 16·06 | 1·0 | 450 | 457 |
| 10·51 | | 510 | 510 |
| 6·23 | | 555 | 559 |
| Ammonium Sulphate— | | | |
| 18·96 | 0·77 | 475 | 470 |
| 10·17 | | 542 | 540 |
| 5·5 | | 575 | 580 |
| Potassium Ferrocyanide— | | | |
| 16·4 | 0·62 | 512 | 513 |
| 10·4 | | 554 | 556 |
| 5·0 | | 605 | 602 |
| Barium Chloride— | | | |
| 16·0 | 0·52 | 530 | 534 |
| 10·0 | | 573 | 573 |
| 4·8 | | 612 | 611 |
| Potassium Iodide— | | | |
| 17·01 | 0·29 | 576 | 576 |
| 10·25 | | 602 | 603 |
| 4·5 | | 627 | 629 |

As stated in my previous note, my formula agrees extremely well with the recent determinations of Amagat, of compression of water up to 3000 atmospheres. But the values of A and B which I deduced from them (especially about 12° C.) are somewhat larger than mine, though they bear to one another nearly the same ratio. If I had used his value of B, the coincidences above would not have been sensibly impaired, but the values of $s$ would have come out a little greater.

# Elimination of Powers of Sines and Cosines between Two Equations. By the Hon. Lord M'Laren.

(Read July 17, 1893.)

This paper is an extension to higher powers of the process which I applied to the elimination of $\theta$ from the equations of the Ellipse-Glissette (*Proc. Roy. Soc. Edin.*, xix. p. 89). It depends on the principle that where the highest power or powers of one of the variables are awanting, derived equations can always be formed by eliminating a power of each of the other variables between the two primitives.

Where the quantities to be eliminated are the sine and cosine of a variable angle, $\theta$, if we put $x$ for $\sin\theta$, $y$ for $\cos\theta$, and introduce a homogenic quantity $z$, we have always a third equation, $x^2+y^2-z^2=0$; and by means of this relation we may depress either $x$, $y$, or $z$ in the primitives to the 1st power.

I shall suppose this preliminary operation to be performed and the equations to be given in the form

$$a_1x^n + a_2x^{n-1}y + a_3x^{n-1}z + a_4x^{n-2}yz + a_5x^{n-2}z^2 + a_6x^{n-3}yz^2 + , \&c. = 0.$$

(1) *i.e.*, $0=(a_1x+a_2y)x^{n-1}+(a_3x+a_4y)x^{n-2}z+(a_5x+a_6y)x^{n-3}z^2+ - +(a_lx+a_my+a_nz)z^n$

Similarly,

(2) $0=(b_1x+b_2y)x^{n-1}+(b_3x+b_4y)x^{n-2}z+(b_5x+b_6y)x^{n-3}z^2+ - +(b_lx+b_my+b_nz)z^{n-}$

These equations consist of $2n+1$ terms—*i.e.*, they are of the *order*, $2n+1$—when written in the 1st of the preceding forms. Considering the equations, as written in the 2nd form, it is evident that by following the method of Bézout, we may eliminate powers of $x$ and $z$ in $n-1$ different ways, and thus obtain $n-1$ independent equations of the 2nd dimension in the coefficients, as thus :—

$$\left.\begin{matrix}(L)x^{n-1}+(M)z=0\\(L')x^{n-1}+(M')z=0\end{matrix}\right\}\quad\left.\begin{matrix}(N)x^{n-2}+(P)z^2=0:\\(N')x^{n-2}+(P')z^2=0:\end{matrix}\right\}\quad\ldots\ldots\quad\left.\begin{matrix}(R)x+(S)z^{n-1}=0\\(R')x+(S')z^{n-1}=0\end{matrix}\right\}$$

From these pairs we form the $n-1$ derived equations (*of the $n^{th}$ degree*), $LM'-L'M=0$, $NP'-N'P=0$, &c.

In the equations (1), (2), above written, the quantity $y$ has been reduced to the 1st degree by substitution from (3). But we may also reduce $x$, and again $z$, to the 1st degree by such substitution, and obtain from the transformed primitives two other sets of derived equations of the 2nd dimension and $n^{th}$ degree; the number of possible derived equations being accordingly $3n-3$. But these derived equations are not all independent, and the right way of performing the elimination is to use only the $n-1$ derived equations of one of the sets. Supposing we make use of the derived expressions of the set $LM' - L'M = 0$, &c., formed as above directed from equations from which all powers of $y$ except the 1st have been removed: after removing $y^2$ from the derived expressions, these are to be multiplied by $y$ and $x$, or $y$ and $z$; and we have then $2n-2$ derived equations, being of the degree $n+1$, and of the 2nd dimension in the coefficients.

But as the equations only contain the 1st power of $y$, the order of the required determinant is, as already seen, $2n+1$. Such a determinant may be formed from the six multiples of the original equations, and $2n-3$ of the $2n-2$ derivatives. Also, as the derived equations are of the 2nd dimension, the degree of the eliminant is $6+2(2n-3)[=4n]$, for any two equations of the like degree, $n$, in $\sin\theta$ and $\cos\theta$.

In order that we may be assured that the derived equations inserted in the determinant are independent, so that the eliminant shall not vanish identically, the following condition suffices:—

If we denote by $y$ the quantity which is reduced to the 1st power by preliminary transformation, then, *first*, each of the derived equations is to be multiplied by $y$, and in these multiples $y^2$ is to be replaced by $z^2-x^2$; *secondly*, the same derived equations are to be multiplied by $x$ or $z$, symmetrically.

The manner of forming the derived equations, and the selection of the most suitable multiples for insertion in a symmetrical manner in the proposed determinant, will be more readily apprehended from the following example, in which I shall find the eliminant (16th degree) of two complete equations in $\sin\theta$ and $\cos\theta$ of the 4th degree.

I shall suppose the 2nd, 3rd, and 4th powers of $y$ to be removed by expressing $y^2$ and $y^4$ in terms of $z^2-x^2$, and the equations to be given in the form shown in the first page of this paper. In order to form the required derivatives, the original equations may be written in the three forms—

$$\left.\begin{matrix}\{a_1x^3 + a_2x^2y + a_3x^2z + a_4xyz + a_5xz^2 + a_6yz^2 + a_7z^3\}x + \{a_8y + a_9z\}z^3 = 0 \\ \{b_1x^3 + \&c.\}x + \{b_8y + b_9z\}z^3 = 0\end{matrix}\right\}$$

$$\left.\begin{matrix}\{a_1x^2 + a_2xy + a_3xz + a_4yz\}x^2 + \{a_5x^2 + a_6xy + a_7xz + a_8yz + a_9z^2\}z^2 = 0 \\ \{b_1x^2 + \&c.\}x^2 + \{b_5x^2 + \&c.\}z^2 = 0\end{matrix}\right\}$$

$$\left.\begin{matrix}\{a_1x + a_2y\}x^3 + \{a_3x^3 + a_4x^2y + a_5x^2z + a_6xyz + a_7xz^2 + a_8yz^2 + a_9z^3\}z = 0 \\ \{b_1x + b_2y\}x^3 + \{b_3x^3 + \&c.\}z = 0\end{matrix}\right\}$$

By cross-multiplication each of these pairs furnishes a derived equation of the 4th degree in which $y^2$ is to be replaced by $z^2 - x^2$. The resulting equations, which follow, are expressed in the usual abridged notation where, *e.g.*, the first term of the expression (6), $\left\{\begin{matrix}a_1,b_3 \\ a_4,b_2\end{matrix}\right\}$, is to be read

$$\{(a_1b_3 - a_3b_1) + (a_4b_2 - a_2b_4)\}x^4 ,$$

and so throughout the paper.

$$\begin{array}{cccccccccc}
x^4 & x^3y & x^3z & x^2yz & x^2z^2 & xyz^2 & xz^3 & yz^3 & z^4 & \\
\{a_2,b_8\} & \{a_8,b_1\} & \left\{\begin{matrix}a_9,b_1\\a_4,b_8\end{matrix}\right\} & \left\{\begin{matrix}a_9,b_2\\a_8,b_3\end{matrix}\right\} & \left\{\begin{matrix}a_9,b_3\\a_8,b_2\\a_6,b_8\end{matrix}\right\} & \left\{\begin{matrix}a_9,b_4\\a_8,b_5\end{matrix}\right\} & \left\{\begin{matrix}a_9,b_5\\a_8,b_4\end{matrix}\right\} & \left\{\begin{matrix}a_9,b_6\\a_8,b_7\end{matrix}\right\} & \left\{\begin{matrix}a_9,b_7\\a_8,b_6\end{matrix}\right\} & (4) \\
\left\{\begin{matrix}a_1,b_5\\a_6,b_2\\a_6,b_3\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_6\\a_2,b_5\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_7\\a_8,b_2\\a_3,b_5\\a_6,b_4\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_8\\a_2,b_7\\a_4,b_5\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_9\\a_2,b_6\\a_3,b_6\\a_3,b_7\\a_8,b_4\end{matrix}\right\} & \left\{\begin{matrix}a_2,b_9\\a_3,b_8\\a_4,b_7\end{matrix}\right\} & \left\{\begin{matrix}a_2,b_8\\a_3,b_9\\a_4,b_6\end{matrix}\right\} & \{a_4,b_9\} & \{a_4,b_8\} & (5) \\
\left\{\begin{matrix}a_1,b_3\\a_4,b_2\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_4\\a_2,b_3\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_5\\a_6,b_2\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_6\\a_2,b_5\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_7\\a_2,b_4\\a_8,b_2\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_8\\a_2,b_7\end{matrix}\right\} & \left\{\begin{matrix}a_1,b_9\\a_2,b_6\end{matrix}\right\} & \{a_2,b_9\} & \{a_2,b_8\} & (6)
\end{array}$$

(4) is to be multiplied by $y$ and $z$, (5) is to be multiplied by $y$ only, and (6) is to be multiplied by $x$ and $y$. These five multiples and the six multiples of the original equations being arranged in determinant form as given below, it can be proved by a very simple test that the eliminant does not vanish identically. In the diagonal from right to left the terms underlined furnish the element $a_1{}^8b_9{}^8$ (by taking the negative part of the expressions from the 4th and 5th rows, and the positive parts from the 6th, 7th, and 8th rows), and it is evident that there is no other such term, because in the first and last three lines $a_1$ and $b_9$ occur once only, and in each of the intermediate lines $a_1b_9$ occurs once only. The same proof applies to the eliminant of two equations in $\theta$ of the 3rd degree printed in the page following, if the lines be arranged in suitable order.

*Eliminant of the 4th Degree in $\sin\theta$, $\cos\theta$.*

| | $X^5$ | $X^4Y$ | $X^4Z$ | $X^3YZ$ | $X^3Z^2$ | $X^2YZ^2$ | $X^2Z^3$ | $XYZ^3$ | $XZ^4$ | $YZ^4$ | $Z^5$ | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| (1) $x$. | $\underline{a_1}$ | $a_2$ | $a_3$ | $a_4$ | $a_5$ | $a_6$ | $a_7$ | $a_8$ | $a_9$ | . | . | |
| (1) $y$. | $-a_2$ | $\underline{a_1}$ | $-a_4$ | $a_3$ | $a_2-a_6$ | $a_5$ | $a_4-a_8$ | $a_7$ | $a_6$ | $a_9$ | $a_8$ | |
| (1) $z$. | . | . | $\underline{a_1}$ | $a_2$ | $a_3$ | $a_4$ | $a_5$ | $a_6$ | $a_7$ | $a_8$ | $a_9$ | |
| (4) $y$. | $\{a_1,b_8\}$ | $\{a_2,b_8\}$ | $\begin{Bmatrix}a_2,b_9\\a_3,b_8\end{Bmatrix}$ | $\begin{Bmatrix}\underline{a_9,b_1}\\a_4,b_8\end{Bmatrix}$ | $\begin{Bmatrix}a_8,b_1\\a_4,b_9\\a_5,b_8\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_3\\a_8,b_2\\a_6,b_8\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_2\\a_8,b_3\\a_6,b_9\\a_8,b_7\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_5\\a_8,b_4\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_4\\a_8,b_5\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_7\\a_8,b_6\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_6\\a_8,b_7\end{Bmatrix}$ | |
| (4) $z$. | . | . | $\{a_2,b_8\}$ | $\{a_8,b_1\}$ | $\begin{Bmatrix}\underline{a_9,b_1}\\a_4,b_8\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_2\\a_8,b_3\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_3\\a_8,b_2\\a_6,b_8\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_4\\a_8,b_5\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_5\\a_8,b_4\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_6\\a_8,b_7\end{Bmatrix}$ | $\begin{Bmatrix}a_9,b_7\\a_8,b_6\end{Bmatrix}$ | |
| (5) $y$. | $\begin{Bmatrix}a_6,b_1\\a_5,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_5\\a_6,b_2\\a_6,b_3\end{Bmatrix}$ | $\begin{Bmatrix}a_8,b_1\\a_7,b_2\\a_5,b_4\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_7\\a_8,b_2\\a_3,b_5\\a_6,b_4\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_6\\a_2,b_5\\a_9,b_2\\a_8,b_3\\a_7,b_4\end{Bmatrix}$ | $\begin{Bmatrix}\underline{a_1,b_9}\\a_2,b_6\\a_3,b_6\\a_3,b_7\\a_8,b_4\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_8\\a_2,b_7\\a_4,b_5\\a_9,b_4\end{Bmatrix}$ | $\begin{Bmatrix}a_2,b_8\\a_3,b_9\\a_4,b_6\end{Bmatrix}$ | $\begin{Bmatrix}a_2,b_9\\a_3,b_8\\a_4,b_7\end{Bmatrix}$ | $\{a_4,b_8\}$ | $\{a_4,b_9\}$ | $=0$ |
| (6) $x$. | $\begin{Bmatrix}a_1,b_3\\a_4,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_4\\a_2,b_3\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_5\\a_6,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_6\\a_2,b_5\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_7\\a_2,b_4\\a_8,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_8\\a_2,b_7\end{Bmatrix}$ | $\begin{Bmatrix}\underline{a_1,b_9}\\a_2,b_6\end{Bmatrix}$ | $\{a_2,b_9\}$ | $\{a_2,b_8\}$ | . | . | |
| (6) $y$. | $\begin{Bmatrix}a_4,b_1\\a_3,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_3\\a_4,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_6,b_1\\a_5,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_5\\a_6,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_4\\a_2,b_3\\a_8,b_1\\a_7,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_7\\a_2,b_4\\a_8,b_2\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_6\\a_2,b_5\\a_9,b_2\end{Bmatrix}$ | $\begin{Bmatrix}\underline{a_1,b_9}\\a_2,b_6\end{Bmatrix}$ | $\begin{Bmatrix}a_1,b_8\\a_2,b_7\end{Bmatrix}$ | $\{a_2,b_8\}$ | $\{a_2,b_9\}$ | |
| (2) $x$. | $b_1$ | $b_2$ | $b_3$ | $b_4$ | $b_5$ | $b_6$ | $b_7$ | $b_8$ | $\underline{b_9}$ | . | . | |
| (2) $y$. | $-b_2$ | $b_1$ | $-b_4$ | $b_3$ | $b_2-b_6$ | $b_5$ | $b_4-b_8$ | $b_7$ | $b_6$ | $\underline{b_9}$ | $b_8$ | |
| (2) $z$. | . | . | $b_1$ | $b_2$ | $b_3$ | $b_4$ | $b_5$ | $b_6$ | $b_7$ | $b_8$ | $\underline{b_9}$ | |

*Eliminant of the 3rd Degree in sin θ, cos θ.*

| | $X^4$ | $X^3Y$ | $X^3Z$ | $X^2YZ$ | $X^2Z^2$ | $XYZ^2$ | $XZ^5$ | $YZ^3$ | $Z^4$ |
|---|---|---|---|---|---|---|---|---|---|
| (1) $x$. | $\underline{a_1}$ | $a_2$ | $a_3$ | $a_4$ | $a_5$ | $a_6$ | $a_7$ | . | . |
| (1) $y$. | $-a_2$ | $\underline{a_1}$ | $-a$ | $a_3$ | $a_2-a_6$ | $a_5$ | $a_4$ | $a_7$ | $a_6$ |
| (1) $z$. | . | . | $\underline{a_1}$ | $a_2$ | $a_3$ | $a_4$ | $a_5$ | $a_6$ | $a_7$ |
| (2) $x$. | $b_1$ | $b_2$ | $b_3$ | $b_4$ | $b_5$ | $b_6$ | $\underline{b_7}$ | . | . |
| (2) $y$. | $-b_2$ | $b_1$ | $-b_4$ | $b_3$ | $b_2-b_6$ | $b_5$ | $b_4$ | $\underline{b_7}$ | $b_6$ |
| (2) $z$. | . | . | $b_1$ | $b_2$ | $b_3$ | $b_4$ | $b_5$ | $b_6$ | $\underline{b_7}$ |
| (α) $x$. | $\left\{\begin{matrix}a_1,b_3\\a_4,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}a_1,b_4\\a_2,b_3\end{matrix}\right\}$ | $\left\{\begin{matrix}a_1,b_5\\a_6,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}a_1,b_6\\a_2,b_5\end{matrix}\right\}$ | $\left\{\begin{matrix}\underline{a_1,b_7}\\a_2,b_4\end{matrix}\right\}$ | $\{a_2,b_7\}$ | $\{a_2,b_6\}$ | . | . |
| (α) $y$. | $\left\{\begin{matrix}a_4,b_1\\a_3,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}a_1,b_3\\a_4,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}a_6,b_1\\a_5,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}a_5,b_1\\a_2,b_6\end{matrix}\right\}$ | $\left\{\begin{matrix}a_1,b_4\\a_2,b_3\\a_7,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}\underline{a_1,b_7}\\a_2,b_4\end{matrix}\right\}$ | $\left\{\begin{matrix}a_1,b_6\\a_2,b_5\end{matrix}\right\}$ | $\{a_2,b_6\}$ | $\{a_2,b_7\}$ |
| (β) $y$. | $\{a_1,b_6\}$ | $\{a_2,b_6\}$ | $\left\{\begin{matrix}a_3,b_6\\a_2,b_7\end{matrix}\right\}$ | $\left\{\begin{matrix}\underline{a_7,b_1}\\a_4,b_6\end{matrix}\right\}$ | $\left\{\begin{matrix}a_6,b_1\\a_5,b_6\\a_4,b_7\end{matrix}\right\}$ | $\left\{\begin{matrix}a_7,b_5\\a_6,b_2\end{matrix}\right\}$ | $\left\{\begin{matrix}a_7,b_2\\a_6,b_3\end{matrix}\right\}$ | $\left\{\begin{matrix}a_7,b_5\\a_6,b_4\end{matrix}\right\}$ | $\left\{\begin{matrix}a_6,b_5\\a_7,b_4\end{matrix}\right\}$ |

$= 0$

It has been shown for two cases, *i.e.*, for the eliminants of equations of the 3rd and 4th degrees, that the solutions are not illusory, because the diagonal contains a term of the form, $a_1^{2n}b_{2n+1}^{2n}$, which does not vanish. This proof may be generalised as follows:—

Supposing the two equations of the $n^{th}$ degree transformed by the removal of all powers of $y$ except the first, we may write,

$$\left.\begin{array}{l} x\{a_1x^{n-1}+\&c.\}+\{a_{2n}y\ +a_{2n+1}z\}z^{n-1}=0 \\ x\{b_1x^{n-1}+\&c.\}+\{b_{2n}y\ +b_{2n+1}z\}z^{n-1}=0 \end{array}\right\}$$

$$\left.\begin{array}{l} x^2\{a_1x^{n-2}+\&c.\}+\{a_mx^2\ \ldots\ +a_{2n+1}z^2\}z^{n-2}=0 \\ x^2\{b_1x^{n-2}+\&c.\}+\{b_mx^2\ \ldots\ +b_{2n+1}z^2\}z^{n-2}=0 \end{array}\right\}$$

$$\left.\begin{array}{l} x^3\{a_1x^{n-3}+\&c.\}+\{a_kx^3\ \ldots\ +a_{2n+1}z^3\}z^{n-3} \\ x^3\{b_1x^{n-3}+\&c.\}+\{b_kx^3\ \ldots\ +b_{2n+1}z^3\}z^{n-3} \end{array}\right\}$$

&c. &c. &c.

In the series of derived equations which are formed by the cross-multiplication of these pairs of equations (eliminating the quantities outside the brackets), we have from the eliminant of the first pair a term $(a_1,b_{2n+1})x^{n-1}z$; whence multiplying by $y$ and $z$, we form two equations containing respectively the terms,

$$(a_1,b_{2n+1})x^{n-1}yz\ ;\ (a_1,b_{2n+1})x^{n-1}z^2\,.$$

Similarly from the eliminant of the second pair, after multiplying by $y$ and $z$, we have two equations containing respectively the terms

$$(a_1,b_{2n+1})x^{n-2}yz^2\ ;\ (a_1,b_{2n+1})x^{n-2}z^3\ ;\ \text{and so on.}$$

Comparing these terms in order with the terms of an equation of the degree, $n+1$, arranged in the order given in the first page of this paper, we see that the constituents $(a_1,b_{2n+1})$ of each successive line fall on successive columns; the whole forming a diagonal which wants only the places of the first and last three lines and columns, and these are supplied, as in the example given, by the multiples of the original equations. Thus, the eliminant of the two complete $n-ic$ equations contains a term $a_1^3b_{2n+1}^3(a_1,b_{2n+1})^{2n-3}$, and thus includes the unique term, $a_1^{2n}b_{2n+1}^{2n}$. Therefore, the eliminant does not vanish identically.

Eliminants expressed in the form of determinants when formed according to the method above described, may be tested for errors in a very simple way, as follows:—

Referring to the complete eliminant of the 4th degree, given above, if we suppose the four coefficients whose suffixes are 8 and 9 to become zero, every term in the Head Line is divisible by $x$, the original equations fall one degree, and therefore if the operation of forming the determinant has been correctly performed, the tenth and eleventh columns ought to vanish, and the determinant ought to fall to the 9th order. By merely looking down the columns, as given, we see that this is so, and that by withdrawing the lines 5 and 6 the diagonal from right to left furnishes a term, $a_1{}^6b_7{}^6$, which does not vanish.

Again, if we suppose the coefficients whose suffixes are 6, 7, 8, and 9 to become zero, the remaining terms in the Head Line are divisible by $x^2$, the last four columns vanish, as they ought to do, and by withdrawing the lines 5, 6, 7, and 8, the determinant is reduced to the 7th order, giving a term $a_1{}^4b_5{}^4$ in the diagonal, which does not vanish.

Lastly, if we suppose the coefficients whose suffixes are 4, 5, 6, 7, 8, 9 to vanish, the determinant reduces to the 5th order and degree, and the expression, as given, also satisfies this test.

If we propose to find the eliminant of two equations in $\sin\theta$, $\cos\theta$, from a determinant of an order higher than $2n+1$, we shall get an eliminant of the degree $4n$, as before, but with greater labour, and in a more inconvenient form.

Thus, in the case considered of two equations of the 4th degree, if we begin by raising the given equations to the 6th degree, and then express $y^2$, $y^4$, and $y^6$ in terms of $z^2-x^2$, the determinant will be of the 13th order $[2(n+2)+1]$. But we shall only be able to use five of the six multiples of each equation, since, if $(\phi)$ denote one of the given equations, evidently the product $(\phi)y^2$, after reduction, is identical with $(\phi)z^2-(\phi)x^2$. The determinant will then consist of ten lines of the 1st dimension and three lines of the 2nd dimension in the coefficients, the eliminant being, as before, of the 16th degree. So, if we raise the given equations three degrees, the determinant, after reduction, will be of the 15th order; and as we can only use seven of the multiples of each equation, there will be fourteen lines of the 1st dimension and one line of the 2nd dimension, the eliminant being of the degree 16, or $4n$, as it should be.

*For equations of different degrees*, say the 2nd and the 3rd, the easiest way of obtaining derived equations is to multiply the lower equation by $x$, $y$, and $z$, and then to transform the three multiples so as to remove the 2nd and 3rd powers of $z$. Each of the transformed multiples in combination with the higher equation will then furnish an independent partial eliminant. Thus from the complete quadratic (A) here given, we form the three multiples $(a')$, $(b')$, $(c')$, and then by removing $z^2$ we form the expressions $(a)$, $(b)$, $(c)$, as thus :—

$$a_1x^2 + a_2xy + a_3y^2 + a_4xz + a_5yz + a_6z^2 = 0 \quad \ldots \ldots \quad (A)$$

---

$$\left.\begin{aligned} &a_1x^3 + a_2x^2y + a_3xy^2 + a_4x^2z + a_5xyz + (a_6x)z^2 = 0 \quad \ldots \quad (a') \\ &a_1x^2y + a_2xy^2 + a_3y^3 + a_4xyz + a_5y^2z + (a_6y)z^2 = 0 \quad \ldots \quad (b') \\ &a_1x^2z + a_2xyz + a_3y^2z + (a_4x + a_5y + a_6z)z^2 = 0 \quad \ldots \quad (c') \end{aligned}\right\}$$

$$\left.\begin{aligned} &\{(a_1 + a_6)x + a_4z\}x^2 + \{a_2x^2 + (a_3 + a_6)xy + a_5xz\}y = 0 \quad \ldots \quad (a) \\ &\{a_1 + a_6\}y.x^2 + \{a_2xy + (a_3 + a_6)y^2 + a_4xz + a_5yz\}y = 0 \quad \ldots \quad (b) \\ &\{a_4x + (a_1 + a_6)z\}x^2 + \{a_5x^2 + a_4xy + a_5y^2 + a_2xz + (a_3 + a_6)yz\}y = 0 \quad \ldots \quad (c) \end{aligned}\right\}$$

I next write the original cubic (B), in which the 2nd and 3rd powers of $z$ are supposed to be removed by substitution, and then the three derived equations formed respectively by combining (B) and $(a)$; (B) and $(b)$; (B) and $(c)$; putting $\alpha_1$ for $a_1 + a_6$; and $\alpha_3$ for $a_3 + a_6$—

$$\{b_1x + b_2z\}x^2 + \{b_3x^2 + b_4xy + b_5y^2 + b_6xz + b_7yz\}y = 0 \quad \ldots \quad (B)$$

---

$$\left.\begin{aligned} &(\alpha_1x + a_4z)(b_3x^2 + b_4xy + b_5y^2 + b_6xz + b_7yz) - (a_2x^2 + \alpha_3xy \\ &\qquad + a_5xz)(b_1x + b_2z) = 0 \quad \ldots \quad (1) \\ &(\alpha_1y)(b_3x^2 + b_4xy + b_5y^2 + b_6xz + b_7yz) - (a_2xy + \alpha_3y^2 \\ &\qquad + a_4xz + a_5yz)(b_1x + b_2z) = 0 \quad \ldots \quad (2) \\ &(a_4x + \alpha_1z)(b_3x^2 + b_4xy + b_5y^2 + b_6xz + b_7yz) - (a_5x^2 + a_4xy + a_5y^2 \\ &\qquad + a_2xz + \alpha_3yz)(b_1x + b_2z) = 0 \quad \ldots \quad (3) \end{aligned}\right\}$$

The determinant is—

| $x^3$ | $x^2y$ | $xy^2$ | $y^3$ | $x^2z$ | $xyz$ | $y^2z$ | $xz^2$ | $yz^2$ | $z^3$ |
|---|---|---|---|---|---|---|---|---|---|
| $\{a_1b_3-a_2b_1\}$ | $\{a_1b_4-a_3b_1\}$ | $\underline{a_1b_5}$ | $0$ | $\left\{\begin{matrix} a_1b_6+a_4b_3 \\ -a_5b_1-a_2b_2 \end{matrix}\right\}$ | $\left\{\begin{matrix} a_1b_7+a_4b_4 \\ -a_3b_2 \end{matrix}\right\}$ | $a_4b_5$ | $\{a_4b_6-a_5b_2\}$ | $a_4b_7$ | $0$ |
| $0$ | $\{a_1b_3-a_2b_1\}$ | $\{a_1b_4-a_3b_1\}$ | $\underline{a_1b_5}$ | $-a_4b_1$ | $\left\{\begin{matrix} a_1b_6-a_5b_1 \\ -a_2b_2 \end{matrix}\right\}$ | $\{a_1b_7-a_3b_2\}$ | $\{-a_4b_2\}$ | $\{-a_5b_2\}$ | $0$ |
| $\{a_4b_3-a_5b_1\}$ | $\{a_4b_4-a_4b_1\}$ | $\{a_4b_5-a_5b_1\}$ | $0$ | $\left\{\begin{matrix} a_4b_6+a_1b_3 \\ -a_2b_1-a_5b_2 \end{matrix}\right\}$ | $\left\{\begin{matrix} a_4b_7+a_1b_4 \\ -a_3b_1-a_4b_2 \end{matrix}\right\}$ | $\{\underline{a_1b_5}-a_5b_2\}$ | $\{a_1b_6-a_2b_2\}$ | $\{a_1b_7-a_3b_2\}$ | $0$ |
| $\underline{a_1}$ | $a_2$ | $a_3$ | $0$ | $a_4$ | $a_5$ | $0$ | $a_6$ | $0$ | $0$ |
| $0$ | $\underline{a_1}$ | $a_2$ | $a_3$ | | $a_4$ | $a_5$ | | $a_6$ | |
| $0$ | $0$ | $0$ | $0$ | $\underline{a_1}$ | $a_2$ | $a_3$ | $a_4$ | $a_5$ | $a_6$ |
| $1$ | $0$ | $1$ | $0$ | $0$ | $0$ | $0$ | $\underline{-1}$ | $0$ | $0$ |
| $0$ | $1$ | $0$ | $1$ | $0$ | $0$ | $0$ | $0$ | $\underline{-1}$ | $0$ |
| $0$ | $0$ | $0$ | $0$ | $1$ | $0$ | $1$ | $0$ | $0$ | $\underline{-1}$ |
| $b_1$ | $b_3-b_5$ | $b_4$ | | $b_2$ | $\underline{b_6}$ | $b_7$ | $0$ | $b_5$ | $0$ |

$=0$

The first three lines are the expansions of (1), (2), and (3); the next three lines are the multiples of (A), viz., ($a'$), ($b'$), ($c'$); the next three lines are the multiples of $x^2+y^2-z^2=0$; and the last line is the equation (B), with the term $b_5y^3$ transformed to $b_5yz^2-b_5yx^2$. To prove that the eliminant does not vanish identically. Observing that $a_1$ is $(a_1+a_6)$, the ten marked constituents include a term $a_1{}^6b_5{}^3b_6$. Now there is no other such term, because if we examine the three constituents $a_1b_5$ in their places, it is seen that the term does not occur again in the same line or column with either of them. The determinant is reducible to the 7th order by substituting for the square and cube of one of the quantities.

## Preliminary Account of Natural History Collections made on a Voyage to the Gulf of St Lawrence and Davis Straits. By Mr Alexander Rodger, University College, Dundee. Communicated by Professor D'Arcy W. Thompson.

(Read May 15, 1893.)

[In February 1892, by the kindness of Messrs David Bruce & Co., Dundee, my assistant, Mr Alexander Rodger, obtained a berth on board the whaler "Esquimaux," and proceeded with her on her usual sealing and whaling voyage. Mr Rodger was provided with dredges, tow-nets, and other appliances, and was instructed to use all diligence in the collection of Natural History specimens, both vertebrate and invertebrate. The course of the ship and the business of her people could not be interfered with for such a purpose, but, by his own industry and the great kindness of the master, Captain Jeffery Phillips, Mr Rodger's results have proved very considerable, and compare well with those of some more costly expeditions.—D. W. T.]

The ship, after reaching St John's, made two voyages in quest of seals, the first to the East Coast of Newfoundland between the Funk Islands and the Straits of Belleisle, the second to the West Coast by way of the Gulf of St Lawrence. Many Scotch ships pass every year on these voyages in the close vicinity of the Funk Islands, famous as an old breeding-ground of the Great Auk; but the hurry of the sealing voyage renders it hopeless to expect that they should stop and make search for the numerous remains that are still to be found. On the first sealing trip (10th to 24th March) no invertebrates were collected, but skeletons, skulls, and various viscera were obtained of Hooded Seals (*Cystophora cristata*), Harp seals (*Phoca greenlandica*), Pussies or White-coats, *i.e.*, the young Harps before the change of coat at about three weeks old, Bedlimers, or Harp seals of the first, second, or third season, that is to say, before their breeding period and while the fur is spotted and the harp upon their back is still incomplete. Another stage of the Harp seal is distinguished by the sealers under the name of *Cats*, young seals prematurely born, from 9 to 18 inches long, bearing a fine white fur; this fur is firm in the skin, while that of the Pussy or White-coat is usually already somewhat loose, and the Cats are therefore in request for making stuffed specimens. Two were obtained for this purpose. The White-coats, according to their quality, or, what is much the same thing, according to the date on which they are caught, are graded into several qualities; the younger and finer are cured for imitation seal-skins, the older ones are tanned for fine qualities of leather. Yet another sort is known as a "Lord"; this is a young Harp seal which has lost his mother, and lives upon the ice until he has absorbed all his own fat, and then takes to the water at about the usual time, which is early in April; he is permanently dwarfed, not growing more than about 2 feet long, and he is said never to breed, though his life is not known to be shortened; he never assumes the harp upon his back, but retains the spots of the Bedlimer. The Lords are particularly ferocious. One young specimen was obtained for stuffing. It would be very interesting to obtain authentic skins or skeletons of old individuals of this remarkable dwarf variety, but it would be difficult to do so, as they remain solitary, not going with the herd, and not coming on the ice to breed.

On the second voyage (26th March to 2nd May) the same seals were met with, and further specimens secured. It was on this voyage (31st March) that the young Lord above alluded to was got; at an earlier date it would have been indistinguishable from a White-coat. Both Harp or Greenland seals and Hooded seals are exceedingly numerous, though the former are of course the more so. The "Esquimaux's" total catch on these two trips (an exceptionally successful season) consisted of 21,076 young Harps, 5,491 Bedlimer Harps, 7,499 old Harps, but only 29 young Hoods and 27 old Hoods. In other cases the Hoods may be caught in many hundreds; this year the s.s. "Esquimaux" has only secured about 800 seals, but these are mostly Hoods. In 1891 the total catch of the fleet was 343,503, and for several years past the total catch has stood at about the same figure, however great the fluctuation in success of individual ships. The total catch of the Newfoundland sealing fleet in 1892 was 358,984 seals.

On this second voyage one specimen was obtained (the only one seen) of the Bearded or Square-flippered seal (*Erignathus barbatus*). This seal, known as the Square-flipper by the Newfoundland sealers, is known to the Scotch whalers by its Esquimaux name of the *Okduke.* It is the shape of the fore-foot that gives this seal its Newfoundland name. No specimens of *Phoca vitulina* were seen, though plenty are got, even in the harbour at St John's, later in the year, when the coast is clear of ice. Nor were any specimens of the Floe-rat (*Pagomys fœtidus*) seen, though this species is occasionally met with, but less commonly than in Davis Straits. The only parasites found were numerous Nematode worms in the stomachs of the Harp and Hooded seals.

On 4th April swabs and a deep-water tow-net were sent down in 100 fathoms water, on a rocky bottom, 20 miles N. by W. of St Paul's Island, in the Gulf of St Lawrence.

Echinoderms were numerous, especially the common *Strongylocentrotus dröbachiensis;* several specimens occurred of *Echinarachnius parma*, a species not met with further north; and three specimens of a fine Astrophyton, probably *A. eucnemis.* The northern form of our common *Buccinum*, *B. greenlandicum*, was here in company with *Cardita borealis*, *Trophon truncatus*, and *Astarte sulcata*, var. *compressa.* *Pycnogonum littorale* was found here, the depth being an unusual one for this species.

Five days later, the ship being fixed in the ice and drifting southward in the current, a small trawl and swabs were sent down in 60 fathoms, again on a rocky bottom, off Norman's Light, in the Straits of Belleisle, 10 miles from the Labrador coast.

On the 17th, the ship being still fast in the ice, a mile further to the eastward, the swabs were again worked, the trawl having been lost meanwhile.

Numerous Hydroids and Polyzoa were got here, including *Campanularia verticillata*, *Salacia abietina*, *Thuiaria thuja*, *Sertularia polyzonias*, var. *gigantea*, and many other forms not yet identified. Several specimens of a fine *Voehringia* occurred. Pycnogons were abundant, and, indeed, throughout the voyage the swabs were successful in catching a particularly large number of these animals. The species here included *Nymphon grossipes*, *mixtum* and *brevitarse*, and *Pseudopallene circularis.* In addition to the former Mollusca, there were here *Cardium elegantulum*, *Margarita* *cinerea*, *Acrissa eschrichtii*, *Pleurotoma exarata*, and *Aporrhais occidentalis*. The Amphipod *Stegocephalus inflatus* was in great numbers. The great *Mysideis grandis*, Goës, represented the Schizopods.

On 19th May, after refitting in St John's, the ship sailed on her usual course to the northward and westward, in order to proceed up the eastern shore of Davis Straits, and thereafter to descend by the western. This course is invariably taken, because at this time of year, and for three months after, the western coast is ice-bound by the floe ice coming down Davis Straits from Smith's Sound and Lancaster Sound, while, with favourable winds, the eastern side is already comparatively clear. No obstruction from ice was met with until Melville Bay was reached on 6th June.

The first collecting done in Davis Straits was on 30th May, Reef-Coal Hill bearing S.E. (magn.) 20 miles, where swabs were sent down in 30 fathoms on a rocky bottom, the ship lying to with contrary winds. A very large number of Amphipods were got here, including such characteristic northern forms as *Amathilla sabini*, *Anonyx nugax*, *Acanthozone cuspidata*, and *Stegocephalus inflatus*. Large specimens of *Boreonymphon robustum* occurred; and among the Mollusca, *Tellina calcarea*, *Astarte borealis*, *Natica clausa*, and *Mya truncata*, var. *uddevalensis*. Four specimens of apparently a species of *Pelonaia* are noteworthy.

On this date several Walrus were met with, and the skeleton of a large male was obtained. In the stomach of the same animal a number of Nematode worms were found.

On 1st June, the ship fast with ice-anchors in Godhavn or Lievely Harbour, Disco Island, a surface net was worked from the dingey with little result, the harbour being choked with ice. The swabs also produced little, although this locality was found very productive by the naturalists of the "Valorous" Expedition, who had visited it at a later date when clear of ice. Here Inspector Anderson, the Danish overseer of Northern Greenland, presented us with four bottles of specimens in spirit.

Two days later, in the land-ice off Upernavik, a tow-net was baited and sunk to the bottom in 50 fathoms water, and brought up an enormous mass of a small Amphipod not yet identified.

The fauna here was very similar to the last station, the same Amphipods and Molluscs occurring in numbers, and four more specimens of the same *Pelonaia*. *Anonyx nugax* was very abundant. Additional Molluscs were *Buccinum tenue*, Gray, *B. finmarchicum*, and *Modiolaria lævigata*. A very remarkable elongated variety of *Balanus crenatus*, lately described by Sars, was present in company with the ordinary form of the same species.

Three days later the first polar bear was obtained, though one had already been found on the N.W. side of Newfoundland during the sealing voyage. In the next few days skeletons were prepared of the Ivory gull, Fulmar petrel, Buffon's skua, kittiwake, and Brünnich's guillemot (Little Auk).

On the 16th, off Cape Franshaw, Beham Martin Mountains bearing 20 miles W., no bottom could be found for dredging (only 400 fathoms of line being available).

Continuing on a course down the west side of Davis Straits, surface-nettings were obtained off Graham Moore's Mountains and Pond's Inlet, and yielded many forms on which we are not yet able to report.

In Pond's Inlet the first Narwhals were caught, and skeletons of a female and full-grown fœtus were obtained.

*Cyamus monodontis* and *C. nodosus* were abundant on the Narwhals, clinging to the numerous scars and wounds upon their bodies, and round the base of the tusk in the males. *C. nodosus* is said to be limited to the latter situation, but I omitted to take note of the fact.

Tow-netting was continued on the following days in the same neighbourhood.

On the 25th, 35 miles off Cape M'Culloch, a sunk and baited tow-net, with swabs, was lowered in 80 fathoms water. *Boreonymphon robustum*, *Chaetonymphon hirtipes*, and *Eurycyde hispida*, all characteristic Arctic species of Pycnogonida, were taken here. A very large *Rhynchonella* occurred, and a number of Gephyrea, including one very curious one, of a very long and attenuated form.

Three days after, a few miles to the southward, the swabs and net were again sunk in 90 fathoms. The *Boreonymphons* that came up had a fine supply of young larvæ adhering to them.

During all this period no whales had been seen by the "Esquimaux," and only one had been procured by the fleet, viz., by the s.s. "Maud," at the end of May, off Disco; nor had we seen any seals, except a few stragglers in the water. On 3rd July a female Okduke was killed, and the skull preserved. On the same date surface-nettings were taken off Pond's Inlet, yielding very large *Clios* and *Limacinas*, *Calanus finmarchicus*, *Euthemisto libellula*, and other forms.

Next day swabbings were taken in 200 fathoms, 10 miles S.W. of Cape Wild. A small *Comatula*, allied to *C. eschrichtii*, but apparently different, *Chaetonymphon macronyx*, not hitherto recorded from westward of the Faröe Islands, an immense *Tanais*, and a very remarkable calcareous sponge, were the chief prizes.

In this neighbourhood *Clio borealis* was very abundant, and specimens were preserved both in spirit and glycerine, the latter keeping very successfully the beautiful colour of the animal.

The ship continued under canvas during the following days, searching unsuccessfully for whales in snowy and misty weather, running too fast for surface-netting or other means of collecting specimens. Then, running northward under steam and canvas, she entered Lancaster Sound and made Elwyn Bay, or White Whale Bay, in Prince Regent's Inlet, on the 12th, in order to prosecute the White whale or Beluga fishery. Lancaster Sound was entirely new ground for the naturalist, so far as concerned the invertebrate fauna. Swabbings and tow-nettings were taken at a depth of 7 fathoms, the water being brackish and muddy. Small Amphipods were so exceedingly abundant that a Floe-rat (*Pagomys fœtidus*) lowered to the bottom was reduced to a clean skeleton in three hours. *Idothea sabini*, an *A. amathilla*, allied to *sabini*, and *Aegina spinifera*, were very abundant. One specimen occurred of a gigantic *Caprella*, about 3 inches long. *Margarita umbilicalis* and *olivacea* were the most interesting Molluscs.

The Beluga fishery was here unsuccessful, though the bay is famous for its White whales, and a great number were got at a later date. Three or four miles up the fjord thousands of skeletons of all ages are said to be lying on the beach. Unfortunately, no further collecting was feasible in Lancaster Sound.

On the 19th the ship was back again in Pond's Inlet, and several Esquimaux dogs' skulls were procured. The very large and remarkable Nematode parasite, *Eustrongylus gigas*, from an Esquimaux dog, had already been obtained from Inspector Anderson at Disco.

The ship was now in the thick of the Narwhal fishery, and 32 individuals were killed. A male skeleton was procured here, as well as four small fœtuses and various anatomical preparations.

On the 21st a small Black whale, a "sucker," about 18 feet long, with whalebone 18 inches long, was captured; the mother was sought for three days, but in vain. The skeleton of this whale was unhappily lost, owing to the anxiety on board to procure the mother, and nothing was obtained from it except a supply of the common parasite *Cyamus ceti*, L.

On the 27th, again running southward, surface-nettings were taken off Erick Point, near Scott's Inlet.

The 30th July found the ship three miles off Coutt's Inlet, and here swabbings were taken at a depth of 130 fathoms on a muddy bottom. This locality was very rich. *Antedon eschrichtii* was abundant, and several specimens of its large and beautiful stalked larva were obtained. Among the Annelids a *Nereis* with a curious flattened tube is remarkable. Besides the Pycnogons previously obtained, we have here *Nymphon elegans* and *sluiteri*, the latter new to Greenland, and an apparently new species.

On 1st August, the ship lying in Hamilton Inlet to take on board a supply of fresh water, a ramble on the shore yielded a number of dead shells and Crustacea, and a small specimen of gold-bearing quartz, for which the locality is known among whaling captains. Among the Crustacea was a specimen of *Arcturus baffini*, in a very large *Idothea sabini*, about 4 inches long.

Next day the pelvic bones were found of a large whale (with 12 feet bone), caught two years before by the s.s. "Aurora."

Two days later the ship was again off Erick Point, which lay 20 miles to the E.S.E., and over a well-known bank in 60 to 100 fathoms water, with a bottom of mud and gravel. The dredge, swabs, and sunk net were sent down. Among the contents were *Nymphon longitarse* and *microrhynchum* (?), both new to Davis Straits; a gigantic *Caprella*, over 3 inches long; the extraordinarily aberrant Isopod *Munnopsis typica*, Sars, only known hitherto from much shallower water; several Cumacea, including *Diastylis goodsiri*, *D. rathkei*, and a form which Professor Sars believes to be a new species allied to the latter, *Astropecten tenuispinus*, far to the northward of its previously recorded range, a small *Selaginopsis*, and many Molluscs, including *Astarte compressa*, *crebricostata*, and *warhami*, and *Trichotropis borealis* and *conica*.

On the 8th a skeleton of an Arctic fox was prepared. Next day the surface net was worked a few miles to the S. of Kater Head.

On the following day, under steam and canvas, the ship was just making her way southward through heavy ice for Cumberland Gulf, in order to resume there the White whale fishing. The fishing ground was not reached until the 19th inst. On entering Cumberland Gulf numerous whales were seen, and many white whales. Passing the Peterhead settlement on Kikerton Island, a depot for oil and skins brought in by the natives, the "Esquimaux" proceeded to the head of the gulf, at which spot the White whale fishery is chiefly prosecuted. The method is as follows:—The White whales come up the gulf in one or more herds with the tide, and the ship, or several ships acting in concert, wait till they get past. The ships then follow the herd, blowing steam-whistles, beating tom-toms, working the injection pumps, and firing guns and rocket guns charged with gravel, until, nicely timing their arrival, the whales are driven across the bar at the mouth of the fjord on the turn of high water. The ship on reaching the bar casts anchor and lowers away all hands, each boat being provided with lances, axes, and guns. Ranged in a line they drive over the bar and right up the fjord, where the whales become stranded, and are massacred in the shallow water. Any that escape for a while find themselves imprisoned within the bar. In this way the "Esquimaux" and the "Aurora" killed between them over 700—by no means an unusual catch. Two skeletons, male and female, and a large number of fœtuses were secured here. None of the many Belugas killed here or in Lancaster Sound were found to bear external parasites, but a number of Cyami from the White whale, and still adhering to the skin, were obtained from Captain Cunningham of the s.s. "Nova Zembla," and are the first authentic specimens from that species. They much resemble, and may be identical with, *C. monodontis*. The young Beluga is of a dark slaty-blue colour, and some individuals were seen which conserved this colour, though nearly full-grown.

While the ship swung at anchor here, in 20 fathoms, a small trawl and swabs were sent down on a muddy bottom and in brackish water. *Arcturus tuberosus*, the spineless variety of *A. baffini*, was in great numbers, together with very large *Idotheas* and *Boreonymphons.* From the same locality came a very singular Ascidian, perhaps allied to *Chelyosoma.*

Unhappily the deeper waters of Cumberland Gulf are still unexplored.

On the 9th September the ship was again in Exeter Harbour, to the north of Cumberland Gulf; and anchored off a deserted English settlement there. Dredgings were conducted in 10 fathoms, on a stony bottom, but the catch was small. *Astyris rosacea* and *Margarita olivacea* were the most interesting forms. Four days later, off Cape Raper, in 60 fathoms, the trawl and swabs were again worked on a bottom of sand and small stones. No less than 10 species of *Pycnogons* were got here, three of which are apparently new to science. Crinoids were in great numbers, and include *Antedon prolixa*, and an allied species, which Canon Norman assures us is new : several stalked larvæ of Crinoids were also got. *Munnopsis typica* was abundant. But the most remarkable find was a single specimen of a *Trichotropis*, kindly identified for us by Canon Norman as *T. bicarinata*; a species only known from the North Pacific.

On the 17th, in Eglinton Harbour, the trawl and swabs were let down in 15 fathoms on a muddy bottom. A handsome Amphipod allied to *Rhachotropis aculeata*, Lep., was found here, and a single specimen of the giant Ostracode *Philomedia bairdi.* Three Schizopods were collected here, *Mysis oculata, M. mixta*, Lilljeb., and an undescribed species. *Nymphon microrhynchum* and an undescribed species represented the Pycnogonida. The Mollusca were very numerous, and included *Crenella decussata, Nucula tenuis, Leda minuta, Serripes greenlandicus, Pleurotoma exarata, Trichotropis borealis*, &c. The Limacinæ were of unusual size.

A period of stormy weather followed, and the "fall" fishing was unsuccessfully prosecuted.

On the 24th October, Erick Point bearing N.W. by W. 10

miles, the swabs were used in 150 fathoms, on rocks. Besides the common *Boreonymphon* and *Eurycyde*, this haul yielded *Chaetonymphon hirtipes*, and *C. macronyx*, and an undescribed species of *Nymphon*. A small *Astrophyton*, a very large *Asterias*, and a specimen of the handsome Decapod *Sclerocrangon salebrosus*, Owen, were also brought up. But the most remarkable form taken was an extraordinary variety of *Strongylocentrotus dröbachiensis*, with enormous pedicellariæ, similar to one, as yet undescribed, which Canon Norman tells us he took last year in Finland.

The swabs were hauled at midnight, and the specimens were thrown into two buckets of sea-water. The whole was frozen solid when I returned after a few minutes' absence. With this little experience of Arctic weather in late autumn, the work of the cruise came to an end.

[*Note.*—The collections of which the above is a preliminary account will be duly reported on in course of time. An account of the *Ophiuridæ*, which we submitted to Mr J. A. Grieg, has been already published in the *Report of the Bergen Museum* for 1892. The mollusca have been identified for us by Dr Chaster of Southport.—D. W. T.]

## Abstract of Paper on the "Rate of Fermentation of Sugars." By W. G. Aitchison Robertson, M.D., D.Sc., F.R.C.P.E. (*From the Physiological Laboratory, University of Edinburgh.*)

(Read January 29, 1894.)

Most of the sugars undergo certain fermentations, the chief of these being the lactic, butyric, and the alcoholic or vinous.

To determine the rate at which fermentation occurs in the commoner varieties of sugar, I performed the following experiments.

### I. Lactic Acid Fermentation.

I made 5 per cent. solutions in distilled water of chemically pure samples of cane sugar, invert sugar,* lactose, dextrose, maltose, and lævulose. 100 c.c. of each of these solutions were placed in separate flasks, and to each of them 10 c.c. of sour skim-milk filtrate were added, and the degree of acidity immediately estimated, calculating it as lactic acid. This I have termed the *initial acidity.* During the succeeding four days the flasks were kept at a uniform temperature of 38° C., which is most suitable for the growth of the *bacterium lactis.* The percentage of acid produced in each solution was estimated at intervals, and the amount of lactic acid formed in definite periods is shown in Table I.

Table I.—*Total Percentage amounts of Acid formed in stated Periods.*

| Hours. | Cane Sugar. | Invert Sugar. | Lactose. | Dextrose. | Maltose. | Lævulose. |
|---|---|---|---|---|---|---|
| 2 | 0 | 0·0018 | 0·0009 | 0.0018 | 0·0009 | 0·0018 |
| 4 | 0 | 0·0031 | 0·0018 | 0·0036 | 0·0012 | 0·0027 |
| 8 | 0·0063 | 0·0090 | 0·0027 | 0·0045 | 0·0036 | 0·0054 |
| 24 | 0·0072 | 0·0135 | 0·0045 | 0·0054 | 0·0045 | 0·0072 |
| 72 | 0·0405 | 0·0423 | 0·0603 | 0·0512 | 0·0315 | 0·2691 |

* The invert sugar was made by rendering the 5 per cent. solution of pure cane sugar slightly acid with sulphuric acid (this is the most powerful acid

A consideration of these figures shows that with—

1. *Cane Sugar* a considerable time elapses before the commencement of its change into lactic acid. It is only after the fourth hour that fermentation begins, and proceeds slowly at first, though after the thirtieth hour the change is rapid and extensive.

2. *Invert Sugar* and the rest of the sugars experimented with, have no resting period like cane sugar, but begin to ferment at once. Up to the thirtieth hour the change which invert sugar undergoes is much more rapid than in the case of cane sugar, but after this period they closely correspond.

3. *Lactose* undergoes a feeble degree of change up to the twenty-fourth hour, after which time, however, the change becomes very rapid.

4. *Dextrose* during the early hours undergoes a slowly progressive fermentation. Later, it becomes rapid, and at the fourth day this sugar has undergone a greater degree of change than any of the other sugars with the exception of lævulose.

5. *Maltose* is apparently less easily and less rapidly fermented than dextrose. Up to the twenty-fourth hour it resembles lactose, but not afterwards, as at the seventy-second hour the latter has yielded almost twice as much lactic acid as the former.

6. *Lævulose* undergoes marked fermentation. Dextrose is much more rapidly changed by the alcoholic ferment than lævulose; but it is precisely the reverse when these sugars are subjected to the lactic acid fermentation. The fermentation of lævulose by the lactic ferment is remarkable in that it yields about five times as much lactic acid as is yielded by any of the other sugars in the same period of time.

in causing inversion). It was then kept at a temperature of 170°–180° F. in a water bath for two hours. After cooling, the loss by evaporation was replaced, and it was rendered neutral by adding a few drops of caustic potash solution. This invert-sugar solution is usually said to consist solely of equal parts of dextrose and lævulose; this, however, is denied by other chemists, as Petit and Maumené.

At the end of the third day the sugars may be arranged in the following order as regards the amount of fermentation which they have undergone :—

1. Lævulose.
2. Lactose.
3. Dextrose.
4. Invert Sugar.
5. Cane Sugar.
6. Maltose.

## II. Butyric Acid Fermentation of Sugars.

This fermentation was induced in the saccharine solutions by adding to each flask of 100 c.c. two grammes of pounded old cheese. Though the resulting change is not a perfectly pure fermentation, yet, for practical purposes, it may be regarded as essentially butyric fermentation.

The results are shown in the following table :—

Table II.—*Total Percentage amounts of Butyric Acid formed in stated Periods.*

| Hours. | Cane Sugar. | Invert Sugar. | Lactose. | Dextrose. | Maltose. | Lævulose. |
|---|---|---|---|---|---|---|
| 2 | 0·0396 | 0·0281 | 0·029 | 0·0312 | 0·0405 | 0·0369 |
| 4 | 0·0449 | 0·0352 | 0·036 | 0·0364 | 0·0431 | 0·0422 |
| 24 | 0·100 | 0·1126 | 0·0774 | 0·1104 | 0·1144 | 0·188 |
| 48 | 0·213 | 0·2393 | 0·1716 | 0·361 | 0·4092 | 0·6212 |

1. *Cane Sugar* appears in this case to undergo rapid inversion, and subsequent fermentation. In the lactic fermentation this sugar had undergone no change by the end of the second hour, as compared with 0·0396 per cent. of acid in this butyric fermentation. At the end of the third day more butyric acid has been formed from cane sugar than from invert sugar.

2. *Invert Sugar* begins at once to undergo butyric fermentation, but more slowly and to a less extent than cane sugar.

3. *Lactose.*—In this sugar also the change seems to begin at once, but the after progress is slow, and never reaches a great amount; the acid forming at the seventy-second hour only 0·2948 per cent. At the same period the solution of cane sugar contains 0·433 and dextrose 1·46 per cent. of butyric acid.

4. *Dextrose* undergoes a slow fermentation until the end of the second day, when the further progress becomes greatly accelerated.

5. *Maltose* suffers change to a greater extent up to the end of the fourth hour than any of the other sugars, with the exception of cane sugar.

6. *Lævulose* until the end of the fourth hour undergoes change, which runs somewhat equally with that of cane sugar. After this period, however, the production of butyric acid from lævulose proceeds at a great rate, till, at the end of the second day, it far exceeds the amount produced from any of the other sugars.

At the end of the second day the sugars may be arranged in the following order as regards the amount of change they undergo when acted on by the butyric ferment:—

1. Lævulose.
2. Maltose.
3. Dextrose.
4. Invert Sugar.
5. Cane Sugar.
6. Lactose.

## III. Alcoholic Fermentation of Sugars.

This was induced by adding 2 c.c. of fresh beer-yeast to each flask containing 100 c.c. of the 5 per cent. solutions of the sugars. The specific gravity was then taken, and all were kept at 38° C. for the following three days. At intervals during this period the specific gravity of each solution was taken, care being exercised to cool them down previously to 15°·5 C.

The following table gives the results:—

TABLE III.—*Degrees of Specific Gravity lost during Alcoholic Fermentation.*

| Hours. | Cane Sugar. | Invert Sugar. | Lactose. | Dextrose. | Maltose. | Lævulose. |
|---|---|---|---|---|---|---|
| 2 | 2·3 | 4 | 0·3 | 3 | 2·7 | 5·3 |
| 4 | 3 | 6 | 0·5 | 4 | 5·2 | 7 |
| 24 | 8·3 | 11·3 | 0·5 | 10·8 | 11·9 | 12·54 |
| 48 | 13·8 | 14·8 | 0·5 | 14 | 17·7 | 12·908 |
| 72 | 14·8 | 17·7 | 0·5 | 14·5 | 17·7 | 12·908 |

1. *Cane Sugar* undergoes a rapid fermentation. At the end of the third day it has not been changed to such an extent as the solution of invert sugar. After being subjected to alcoholic fermentation for 120 hours the specific gravity was 1000·2, and the solution gave only the faintest trace of reduction of the cupric oxide from its alkaline solution.

2. *Invert Sugar* undergoes a steadily progressive fermentation, and reaches its highest limit in the case of this sugar. Maltose resembles this sugar somewhat in its rate of fermentation, and both have suffered an equal loss in specific gravity at the seventy-second hour. The invert sugar solution gave a very faint reduction of cupric oxide 120 hours after the commencement of the experiment.

3. *Lactose* did not undergo alcoholic fermentation at all; the slight loss in specific gravity being probably due to impurity in the sugar.

4. *Dextrose* resembles cane sugar as regards its proneness to undergo the alcoholic fermentation. There was no sugar present in this solution 120 hours after the beginning of the experiment.

5. *Maltose* undergoes a rapid and extensive degree of fermentation, reaching its highest limit in 48 hours.

6. *Lævulose* begins to ferment more rapidly than any of the

other sugars, but falls far short of some of them in the extent to which it is changed.

The following is the order as regards the amount of alcoholic fermentation which the sugars were found to have undergone at the end of the third day:—

1. Maltose.
2. Invert Sugar.
3. Cane Sugar.
4. Dextrose.
5. Lævulose.
6. Lactose (scarcely changed).

## Practical Deductions.

In dyspepsia there is usually much delay in the absorption of carbohydrates, even when digested. They are thus very prone to undergo fermentation, and specially so if the carbohydrate be in slight excess.

In that variety of dyspepsia, accompanied by lactic fermentation, the use of those sugars which rapidly undergo the lactic change—viz., dextrose, lævulose, and invert sugar—is contra-indicated; while the moderate use of cane sugar, maltose, and lactose may be allowed.

In those cases of dyspepsia where butyric fermentation is prominent, milk sugar would seem to be the most suitable, as it is least easily changed by the ferment. On the other hand, maltose is very readily changed, as the other sugars also are to a lesser degree.

Lastly, is dyspepsia associated with the alcoholic and acetic acid fermentations, cane sugar, dextrose, maltose, and lactose may be allowed in small amounts, while invert sugar and lævulose should be forbidden. Lactose, however, is the sugar to give in this condition, as it is not at all acted on by the alcoholic ferment.

These theoretical conclusions have been supported by clinical observation in the case of cane and invert sugar. I leave it for future investigation to show whether they are true as regards the other sugars.

## On the Penguins observed during the Sealing Voyage of the s.s. "Active" in the neighbourhood of Erebus and Terror Gulf. By C. W. Donald, M.B. Communicated by Professor D'Arcy W. Thompson.

(Read July 17, 1893.)

In September 1892 I sailed from Dundee on board the s.s. "Active," which, with three other ships, was sent out to discover, if possible, a whale fishery among the Antarctic ice. The general record of my voyage will be published elsewhere, as will also sundry other observations in natural history. The present note is concerned solely with the species of penguin met with to the south of the Falklands.

By far the most abundant form met with in the far south is the Black-Throated species, *Dasyrhamphus adeliæ* (H. and J.), which, as regards its plumage, is fairly satisfactorily figured by Gray in the voyage of the "Erebus" and "Terror" (pl. xxviii.), although that figure, like most indeed of the published representations of these birds, is far from being correct in general form and contour, the tail, beak, and eye having noticeable differences. This bird was met with on making the ice, in the latitude of the S. Shetlands, and about 30 miles off the land (lat. 61° 14′ S., long. 52° 27′ W.); about a dozen or so were to be seen at a time sitting or lying in twos and threes on the floating cakes of "pan-ice." Passing further to the southward, and nearing the Danger Islands immediately to the east of Joinville Land, the birds increased greatly in number, and were seen in the water in small schools, or sitting on the ice by tens and twenties. We had ample opportunities of watching the peculiar gait and attitudes of the bird, which he shows in common with all his tribe, and which, indeed, have often been described before. Standing absolutely erect, he supports himself on the tripod of feet and tail: as he waddles along, with his feet, as it were, tied together, and trying to balance himself by vigorous movements of his flippers, his tail cuts a deep furrow in the snow, broken at intervals as he half loses his balance and sways forwards; hurrying on, he soon loses his balance altogether and topples forward on to his breast, in which attitude he progresses at an even more rapid pace, the flippers being used alternately as paddles, and the feet pushing behind, the tail in this posture not touching the ground. In the water his modes of progression are also two: usually he is seen to swim under water in a prolonged dive, broken at intervals of about 30 yards, as he rises for breath, leaping clean out of the water to the height of perhaps a foot, and immediately disappearing with scarcely a ripple, after clearing a space of about 2 to $2\frac{1}{2}$ feet. Swimming in this way the feet remain motionless, and only the flippers act as powerful paddles; in this manner the bird shoots along with great rapidity. The other mode of swimming develops but a slow pace; floating on the surface like a cormorant, he swims in the ordinary way by means of his webbed feet, his wings remaining idle. On leaving the water for the ice, he shoots straight up from below the surface, and lands in an erect position; in this way he can jump on to a piece of ice as much as $2\frac{1}{2}$ feet above the water-line.

In Lieutenant Spry's notes on the voyage of the "Challenger,"* he states, as the result of an experiment, that a penguin perished on being held under water for the space of *one and a half minutes.* To test this statement I repeated the experiment, and held a penguin below the surface for the space of 6 minutes. At the end of 2 minutes, among other violent struggles, convulsive pumping movements of the chest occurred; these were repeated at the end of $4\frac{1}{2}$ minutes, and again immediately before I released the bird. Though considerably exhausted, it recovered satisfactorily, and was set at liberty half an hour afterwards. To account for this discrepancy in the two results, I may say that I carefully excluded water from the lungs by compressing the trachea; whereas in Lieutenant Spry's experiment, the bird was simply lowered below the surface in a lobster creel.

On one occasion (January 5), in the north of the Erebus and Terror Gulf, we had an opportunity of seeing the birds swimming in large schools of from 200 to 300, the movements of the school

* *The Cruise of H.M.S. "Challenger,"* by W. J. Spry, R.N., p. 96:—"The bird was found quite dead at the end of 4 minutes, and subsequent experiments showed that $1\frac{1}{2}$ minutes was sufficient to kill it."

being controlled by a single individual which followed in the rear, and which appeared to be of larger size, though we could not approach close enough to determinate its characters. When first seen, at a distance of about 200 yards, the school nearest the ship were leaping and diving noisily; on a croak from the leader this noisy sport instantly ceased, and the whole school swam quietly along for several minutes; in response to another and slightly altered croak, the leaping and diving recommenced; and on a third croak, the whole school disappeared in a prolonged dive.

On the evening of the same day we saw on a piece of ice, some 8 or 10 miles to the south-east, about forty Black-Throated penguins grouped round a pair of large penguins of a different species, possibly identical with those that had directed the schools. One of these was preserved, and is an Emperor penguin in young plumage. On the same piece of ice was a *Chionis* and a seal. It was found over and over again, from inspection of the seal's stomach, that the penguins form the main portion of their diet, but, at the same time, the penguins while on the ice show no fear of the seals; and it is, therefore, probable that they are captured while in the water or during the night. The seals mostly come upon the ice about nine o'clock in the morning, and leave it to feed about seven in the evening.*

Three penguin rookeries were seen in Joinville Land. Two of these were not visited; the other, a very large one, belonged to this species, and was situated on the north shore of the new inlet, named by Captain Robertson the Firth of Tay, in lat. 63° 16′ S., long. 55° 53′ W. I had not the good fortune to land upon this rookery. According to the boat's crew who did so, the birds were in countless multitudes; the nests were crowded together in blocks formed by pathways running nearly at right angles to one another, and the birds were uniformly of the same species. Two eggs from the rookery measured 2·5 by 2 inches, and 2·6 by 2·1 inches.

The cry is seldom heard, and mostly at night or when the birds are disturbed; it is a short, rather harsh "quaugk." Among themselves, when undisturbed, they make a gentle crooning sound. Their food consists mainly of a rather large red shrimp, or rather

* *Cf.* Account in Kerguelen Expedition, *Phil. Trans.*, clxviii.

Schizopod, of the genus *Euphausia*, and the stomachs frequently contained a number of pebbles.

The Black-Throated penguin was seen as far south as lat. 64° 50′, and I have no doubt extended much farther. Within 30 miles of the land they were fairly numerous, but at a greater distance from shore, even in the midst of abundant ice of the same character, they became scarce, and only very few were seen 90 miles from the land.

About the second week in February we began to see, between lat. 62° 30′ and 63° 30′ S., long. 53° 54′ W., large flocks of a White-Throated Penguin of the type described by Dr O. Finsch as *Dasyrhamphus herculis*,* and, at the same time, the Black-Throated form became much scarcer, not more than half a dozen to a dozen being seen during the day. It certainly seemed to me that these White-Throated birds were but the young of *D. adeliæ*, though they must, in this case, have been only about six weeks old. The white throat, the only point in which the plumage differs from the Black-Throated variety, showed in many cases a grey tint, small black feathers shining through among the white. In a skin brought home, and placed in the Museum of University College, Dundee, the lower part of the breast is still downy, the long down being not yet covered by the contour feathers. The bill is somewhat lower and narrower than in the Black-Throated birds; and, further, it may be noted that the tenderness of these White-Throated birds was remarked by all. No rookeries of White-Throated birds were seen. In short, though the presence so early in the season of large flocks of young birds unmixed with older ones is sufficiently remarkable, yet the weight of evidence seems to be in favour of this interpretation, and against the existence of *D. herculis* as an independent species.

We continued to see the White-Throated Penguins until the 24th February, on which date we left the neighbourhood of the ice and sailed northwards for the Falklands. We saw no more penguins of any kind after leaving the ice till we came within a day's sail of the Falklands.

A large penguin was seen in the water on one or two occasions in lat. 63°; several were again seen on ice-floes in lat. 64° 20′, and one

* *Proc. Zool. Soc.*, 1870, p. 322, pl. xxv.

skin obtained here, and brought home in good condition by Captain Fairweather and Mr Bruce of the s.s. "Balæna," proves to be an Emperor penguin, *Aptenodytes forsteri*, G. R. Gray, of great size and beautiful plumage.

The capture of this bird—which was taken on board alive—is vividly described by Mr Bruce. Five men attempted to hold it down on the ice, but were quite incapable of doing so, and got thrown about like so many ninepins. Eventually they got two leather belts strapped round him, but the bird burst those by merely expanding his chest. A stout rope was then fastened round him, his legs, flippers, and neck being all tied together. In spite of this he again got loose while in the small boat. On being taken on board, he nearly stunned the ship's dog by a blow with his flipper.

This skin measured 4 feet 10 inches from the tip of the beak to the extremity of the tail, and the bird itself weighed 74 lb. A specimen brought on board the "Active" weighed 60 lb., and measured 45 inches in vertical height, as it would have stood in the erect position; its greatest circumference was $37\frac{1}{2}$ inches, and the length from tip to tip of the flippers $36\frac{1}{2}$ inches. Very large penguins were seen in the water and on the ice in great numbers by Captain Larsen of the s.s. "Jasen," in lat. 64°, and between the meridians of 47° and 49° W. The stomach of one of the large penguins contained the following heterogeneous mixture: several beaks of a very large cuttle-fish, a number of large schizopodous Crustacea, some fish-bones, and a large quantity of angular pebbles.

The figure of this bird in the Voyage of the "Erebus" and "Terror" is again excellent in regard to colour, but inaccurate in shape.

No specimen of the King penguin, *Aptenodytes pennantii*, G. R. Gray, was brought home, but I am not able to say whether or not the whole of the large individuals seen were really Emperors.

Of the Ringed penguin, *Pygoscelis antarctica*, Forst., we on board the "Active" only saw a single specimen, which we obtained in lat. 64° 12′ S., long. 55° 40′ W., off Seymour Island; but several others were seen by the officers of the "Balæna" on first making the ice.

A sailor on board the "Active" describes another Ringed variety, seen in the same neighbourhood, with a white patch on the top of the head, and the bill and legs of an orange colour, and thus apparently combining the character of the Ringed penguin with those of the White-Headed penguin, *P. papua*, Wagl.

The White-Headed or Johnny penguin was first seen in lat. 63° 37′ S., long. 55° 30′ W., to the east of Paulet Island, and within five miles of the land. Attention was first drawn to the bird by its loud croak, or "quaugk," much harsher and more penetrating than that of the Black-Throated penguin, or indeed than any of the others; the head showed above the water the easily distinguishable and characteristic white crown. On 6th January I landed on a small rookery belonging to the species, in lat. 63° 18′ S., long. 56° 35′ W., at the western extremity of what is now known as Dundee Island to the south of Joinville Land. This rookery was placed about a stone's-throw from high-water mark, on the top of a horse-shoe-shaped hillock. It consisted of some forty nests, grouped irregularly around the horse-shoe. No paths were seen to approach it from the sea, nor could a path well have been beaten down in the hard stony clay which formed the hillock. Each nest was formed of small stones and clayey earth heaped together into the shape of a small conical mound, with a depression some 3 inches deep in the centre; this latter was lined with feathers and down from the parents, most of which showed upon the lower part of their breasts a strip of bare red skin from which the feathers had been pulled. On Kerguelen Island, Mr Sharpe describes the nests of this species as being composed of leaf-stalks and seed-stems of *Pringlea;* but on Dundee Island, a few small tufts of moss formed the whole visible vegetation. Rather more than half the eggs were already hatched, and in many nests one of the two eggs was already hatched, while it was still possible to blow the other. I did not notice a difference in the size of the two eggs, such as is described by Mr Sharpe in the above-quoted paper on Kerguelen. Two eggs brought home are of a chalky-white colour with a faint tinge of blue, and measure 2·7 by 2·2 inches, being thus distinctly larger than those of the Black-Throated penguin. No penguins of other species were noticed in the immediate neighbourhood of this rookery. Opposite on the coast of Joinville Land, a mile and a half to 2 miles away on the other side of the Sound, we saw, though we could not visit, a rookery of immense size, of what appeared, with the aid of a good glass, to be the common Black-Throated penguin.

No crested penguins of any species were seen by us or by the crews of any of the Dundee ships south of the Falklands. But Captain Larsen, of the s.s. "Jasen," saw on the S. Orkneys a rookery of birds which he describes as being intermediate in size between the Emperor and Black-Throated penguins, having a yellow patch under each eye, and a *red* supraciliary crest extending backwards on each side to a length of 3 to 4 inches. Captain Larsen is an acute and careful observer, and took great interest in the natural history of the voyage. He had preserved two specimens of this penguin, but both disappeared, and he thought it not unlikely that they had found their way into the hands of the crew, in which case they may yet be forthcoming. Captain Larsen repeated his description to me on two occasions in identical terms, and I am left with the impression that a new species is probably indicated.

# Preliminary Note on the Hygrometric State of the Atmosphere at Ben Nevis Observatory. By Andrew J. Herbertson. *Communicated by* Dr A. Buchan.

(Read February 6, 1893.)

The present communication consists of a description of the apparatus and methods employed in a series of hygrometric experiments recently made at the Ben Nevis High-Level Observatory, and a very brief notice of some of the results obtained. The investigation here outlined is a necessary preliminary to a more elaborate one, which, it is hoped, may be carried out this summer.

The gravimetric method of estimating the quantity of water vapour in a known volume of air was adopted, and readings of dry and wet bulb thermometers were taken during the experiment.

The apparatus was fitted up at the Low-Level Observatory in Fort-William at the beginning of September 1892, and found to be in working order. It was then transported to the summit station, and used whenever possible until a month ago. Experiments were made in hygrometric conditions varying from saturation to that extreme dryness which is occasionally experienced on Ben Nevis.

*Apparatus.*—A 6-inch U-tube, with a small bulb blown at the bend and an L piece of narrower tubing fused to one arm, was filled

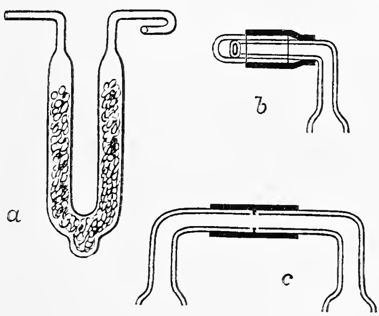



Fig. 1.

with granulated pumice, which had been washed with sulphuric acid and then heated until fumes ceased to be given off. A second piece of narrow tubing, shaped ⊏, was next fused to the free arm (see fig. 1, *a*). Afterwards pure sulphuric acid was sucked in, and allowed to drain off; and the narrow part of the tube was cleaned.

After trying various expedients, the tubes were stopped by a glass cover held in its place by a narrow band of india-rubber, which prevented any leakage (see fig. 1, *b*). The tubes were joined by keeping the free ends close together by means of a piece of tightly-fitting india-rubber tubing (see fig. 1, *c*).

A 14-litre bottle filled with water was used as an aspirator, and also 5-litre reversible metal aspirator made by Muencke of Berlin. When the temperature was below the freezing-point, the latter alone was employed, filled with a mixture of water and methylated spirit in the proportion of three of the former to one of the latter. The balance was a short-beamed one made by Verbeek and Peckholdt of Dresden. It was kindly lent to me for the winter by University College, Dundee. The wet and dry bulb thermometers were those used in the Ben Nevis observations hung in the Stevenson screen. Several check readings were taken from a sling thermometer. In the Table all temperatures have been expressed in centigrade degrees.

*Method.*—Three capped tubes were weighed, one against weights, and each of the other two against this standard tube and weights

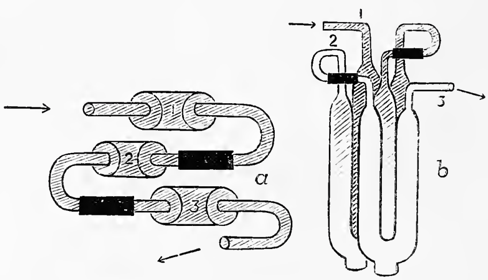



Fig. 2.

combined, except in experiments marked † in the table. The standard tube went through the same experiences as the other tubes, except that the caps were never removed. The other two tubes were then joined in series, and a third tube attached which prevented any vapour from the aspirator reaching the weighed tubes.

The aspirator was set about 5 feet to the windward side of the Stevenson screen, and the box containing the bulbs was placed on the top of the aspirator stand so that the mouth of the tube was about 4 feet from the ground and faced the wind.

The cap was then removed from tube 1, and the aspirator allowed to act. Usually 14 or 15 litres were drawn through the tubes at the rate of 2 litres per minute in the case of the bottle aspirator, and of $\frac{1}{2}$ litre per minute in the case of the reversible one. No difference could be detected between results obtained by aspirating fastly and slowly. In very dry weather 20 and 24 litres of air were sucked through the tubes.

During the experiment the dry and wet bulb thermometers were regularly read, usually every two or every three minutes. In some experiments, when temperature and evaporation conditions were very constant, observations were made every five or six minutes; but, when these varied very rapidly, readings were taken every minute. In frosty weather the wet-bulb required constant attention, and indeed when the air was very dry and the wind strong, a new coating of ice had to be put on every few minutes. The sensitiveness of the wet-bulb under these conditions was remarkable, sometimes varying as much as 0°·7 F. in a minute. It was difficult to determine at times when the bulb was giving the true evaporation. When the readings began to show a steady rise the bulb was moistened, and if, after waiting for some time, the temperature registered before that rise was again reached, it was assumed that such temperature was the true wet-bulb reading. When there was little wind this difficulty was much less, except in a calm, when, of course, the sluggishness of the instrument was great. In future experiments it will be advisable to observe the dry and wet bulb in a current of air of known velocity.

At the time of the earlier experiments the dust-counter was not in working order, but afterwards the number of dust particles was estimated, except when the wind was south-westerly.

The general weather conditions during each experiment were noted, and a copy made of corresponding observations made at the Low-Level Observatory at the same time.

After exposure, the cap was replaced on tube 1. Inside the observatory each tube was capped, and then placed in a box, kept dry by sulphuric acid, until the surface was free from deposit of moisture and the tubes had taken the temperature of the room.

The tubes were then reweighed.

Weighings were made to $\frac{1}{10}$ milligramme; but the impossibility of finding a perfectly satisfactory place for the balance made the conditions of weighing not quite ideal.

The average change of weight in the standard tube, which had no air drawn through during these experiments, was from $+0{\cdot}53$ to $-0{\cdot}57$ milligrammes, and that of the safety tube, omitting the cases where the first tube obviously did not dry the air, was from $+0{\cdot}55$ to $-0{\cdot}52$ milligrammes. The latter figures represent an average gain and also an average loss of less than 0·04 milligrammes per litre of air aspirated.

*Results.*—Nearly a hundred experiments were carried out at the High-Level Observatory, and of these at least half a dozen are valueless owing to breakages, and a score were made in a supersaturated atmosphere. The records of fully sixty experiments are available. From these a diagram has been drawn in which the abscissæ represent temperatures of the dry-bulb thermometer, the ordinates the difference between the temperatures of dry and wet bulb thermometers, and the amount of water is noted for each experiment. On examining the figure it is found that three straight lines can be drawn through points representing 0·5, 1·0, and 2·0 milligrammes of water vapour per litre of air. These are approximately parallel and equidistant. Sufficient data have not yet been obtained to allow these lines to be drawn more precisely, but experiments carried out in summer should give the results needed to extend the present lines and to show their exact form, as well as to increase the number of lines.

Assuming these lines to be parallel and equidistant, their equation is (when $t$ is the temperature of the dry-bulb thermometer in centigrade degrees, $t-t'$ the difference between it and that of the wet-bulb one, and $w$ is the weight of water vapour per litre of air expressed in milligrammes)

$$t - t' = \tfrac{1}{3}(t + 20) - 1{\cdot}8w\,.$$

About twenty-five per cent. of the data used in preparing the diagram do not agree with the lines drawn from this equation. The most glaring exceptions are easily explained, and causes for some other deviations may be given. Experiment 96 gave $t = -3^\circ{\cdot}05$, $t - t' = 3^\circ{\cdot}35$, and $w = 1{\cdot}6$, an amount largely in excess of that given by other experiments for somewhat similar dry and wet bulb readings. On referring to the general weather notes, it was found that a slight drifting of snow began during the experiment. In No. 90, $t = -0^\circ{\cdot}7$, $t - t' = 5^\circ{\cdot}4$, $w = 0{\cdot}86$. In this case the wind blew the smoke from the kitchen chimney towards the Stevenson screen. Again, the effect of a dense haze explains the high value, $w = 3{\cdot}88$, in No. 36 for $t = 2^\circ{\cdot}8$ and $t - t' = 2^\circ{\cdot}35$. Leaving out of account these three cases, the experiments which give results higher than those summarised by these lines are almost equal in number to those whose values are lower. It is worth noticing that in the latter case the dust records are invariably very low, and the air calm or almost so; in the former case the dust particles, while never low, are scarcely above the average, but the wind is usually high. It seems possible that variations in the number of dust particles influence the results, but sufficient data for estimating this influence have not yet been obtained.

*Compared with Hygrometric Tables.*—In comparing the diagram with one made by using similar elements obtained from Guyot's Tables, it is found that the corresponding lines drawn through points of equal weight of water vapour per litre are approximately of the same sort, viz., parallel straight lines; but they cut the $x$-axis at different temperatures from the lines in the figure. How far this may be due to difference of elevation cannot well be determined until experiments have been tried at different altitudes.

The dew-point has been reduced from the observed readings of the dry and wet bulb thermometers by means of Glaisher's Tables, and the amount of water vapour in saturated air, at this temperature and at the summit pressure, compared with observed values. The observations made during the very dry weather at Christmas cannot be used since these tables do not give values for such low dew-points. It would seem that observed values are in excess of tabulated ones below a dry-bulb temperature of about 25° F., while they are in defect above it. This would agree with the results obtained by Mr H. N. Dickson with Professor Chrystal's form of Mr Dine's hygrometer.

The tables used by the different meteorological services for

Table.—*Observations made in Dry Weather at Ben Nevis High-Level Observatory during Winter*, 1892–93.

| No. | Date. | Hour. | Temperature. Dry. | Temperature. Dry-Wet. | Change in Weight of Standard, mgs. | Change in Weight of 2nd Tube, mgs. | Change in Weight of 1st Tube, mgs. | Litres of Air Aspirated. | Mgs. of Water Vap. in each Litre of Air. | Pressure, mm. | Wind, 0–12. | Cloud, 0–10. | Dust, per c.c. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | 1892. | | | | | | | | | | | | |
| 6 | Sept. 10 | 13·34–67 | 0·72 | 0·10 | 0·0 | 16·5 | 92·8 | 20 | 5·47 | 645·8 | W.N.W. 0–2 | Str. 10 | ... |
| 19 | Sept. 21 | 10· 9–31 | 3·00 | 1·05 | −0·1 | +0·6 | 60·0 | 15 | 4·00 | 653·1 | Calm | 0 | ... |
| 20 | ,, | 10· 9–21 | 3·06 | 0·89 | −0·1 | +0·2 | 57·6 | 14 | 4·11 | 653·1 | ,, | 0 | .. |
| 21 | ,, | 11·45–66 | 4·33 | 2·77 | 0·0 | +0·9 | 43·31 | 15 | 2·89 | 653·3 | ,, | 0 | ... |
| 22 | ,, | 12·55–72 | 5·50 | 3·06 | +0·5 | −0·2 | 53·0 | 14 | 3·79 | 653·4 | ,, | 0 | ... |
| 23 | ,, | 13·49–70 | 6·39 | 3·22 | +0·2 | 0·0 | 52·4 | 15 | 3·49 | 653·5 | ,, | 0 | ... |
| 24 | ,, | 13·49–61 | 6·17 | 3·39 | +0·2 | +0·2 | 45·7 | 14 | 3·26 | 653·5 | ,, | 0 | ... |
| 25 | ,, | 15·52–73 | 5·78 | 4·11 | 0·0 | −0·3 | 35·4 | 15 | 2·36 | 653·6 | ,, | 0 | ... |
| 26 | ,, | 15·52–63 | 5·94 | 3·77 | −0·1 | −0·1 | 38·0 | 14 | 2·71 | 653·6 | ,, | 0 | ... |
| 29 | Sept. 22 | 5·57½–79 | 2·67 | 5·89 | +0·4 | −0·4 | 13·9 | 15 | 0·93 | 654·2 | S.E. 0–1 | 0 | ... |
| 30 | ,, | 7·15–29 | 4·00 | 6·44 | +0·4 | +0·3 | 9·7 | 10 | 0·97 | 654·2 | E.S.E. 1 2 | 0 | ... |
| 31 | ,, | 7·15–25½ | 3·83 | 6·28 | +0·6 | −0·1 | 11·1 | 14 | 0·79 | 654·2 | ,, | 0 | ... |
| 32 | ,, | 8·4½–27 | 5·22 | 6·95 | +0·2 | −0·2 | 12·3 | 15 | 0·82 | 654·3 | ,, | 0 | ... |
| 33 | ,, | 16·34½–56 | 6·22 | 6·56 | +0·3 | −0·3 | 23·5 | 15 | 1·57 | 653·3 | S. 1–2 | 0 | ... |
| 34 | ,, | 18·42–59 | 4·66 | 6·56 | 0·0 | −0·4 | 11·2 | 10 | 1·12 | 653·0 | S.S.W. 1–2 | 0 | ... |
| 35 | ,, | 18·42–52 | 4·83 | 6·84 | +0·4 | −0·7 | 13·3 | 14 | 0·95 | 653·0 | ,, | 0 | ... |
| 36 | Sept. 23 | 8·17–41 | 2·78 | 2·33 | −0·1 | −0·4 | 56·6 | 14·6 | 3·88 | 653·0 | S. × E. 3 | Str. 10 | Haze |
| 43 | Oct. 25 | 8·38–65 | −7·67 | 0·44 | +0·1 | 0·0 | 29·6 | 14·9 | 1·99 | 641·0 | Calm | 0 | ... |
| 45† | ,, | 12· 0–29 | −5·56 | 1·00 | +0·4 | +0·9 | 28·4 | 14·8 | 1·92 | 641·6 | ,, | 0 | ... |
| 46 | ,, | 13·16–41 | −5·78 | 0·72 | 0·0 | 0·0 | 31·7 | 14·7 | 2·16 | 641·7 | N. 0–1 | 0 | ... |
| 47 | ,, | 13·42–66 | −6·28 | 0·61 | +0·1 | −0·3 | 30·8 | 14·6 | 2·11 | 641·7 | ,, | 0 | ... |
| 48† | Oct. 26 | 7·23–51 | −5·72 | 1·06 | +0·1 | +0·1 | 31·9 | 14·6 | 2·18 | 643·4 | S.SS.W. 1 | 0 | ... |
| 49† | ,, | 8·33–53 | −5·11 | 1·34 | +0·1 | +0·3 | 21·1 | 9·7 | 2·18 | 643·4 | S.SS.W. 1–2 | 0 | ... |
| 54† | Nov. 23 | 8·47–68 | 4·28 | 4·22 | −0·5 | +0·3 | 26·7 | 12 | 2·22 | 652·5 | S. 2–3 | 0 | ... |
| 55† | ,, | 13·34–55 | 5·06 | 4·39 | +0·8 | +3·0 | 27·9 | 12 | 2·58 | 651·8 | ... | 0 | ... |
| 57† | ,, | 14· 4–12 | 4·89 | 4·28 | +0·8 | +0·2 | 34·5 | 14 | 2·46 | 651·7 | ... | 0 | ... |
| 58† | ,, | 16·44–66 | 2·78 | 3·89 | +0 8 | +1·2 | 24·7 | 11·95 | 2·18 | 651·3 | Calm | 0 | ... |
| 59† | ,, | 16·45–52 | 2·56 | 3·78 | +0·8 | +0·9 | 29·1 | 14 | 2·08 | 651·3 | ,, | 0 | ... |
| 61† | Nov. 24 | 6· 9–16 | 1·61 | 3·78 | −0·6 | +1·3 | 28·6 | 14 | 2·14 | 648·8 | ,, | 0 | ... |
| 62† | ,, | 11·23–30 | 2·56 | 4·39 | −0·4 | −0·2 | 23·5 | 14 | 1·68 | 648·8 | ,, | 0 | 135 |
| 63† | ,, | 11·50–57 | 2·95 | 4·83 | +0·2 | 0·0 | 20·2 | 14 | 1·44 | 648·8 | ,, | 0 | 135 |
| 64† | ,, | 14· 1– 8 | 4·28 | 5·22 | −0·2 | +4·7 | 11·3 | 14 | 1·14 | 648·6 | ,, | 0 | 25 |
| 65† | ,, | 16·23–30 | 1·95 | 4·89 | −1·4 | −1·8 | 15·6 | 14 | 1·11 | 648·7 | ,, | 0 | 111 |
| 66† | ,, | 16·41–48 | 1·56 | 4·72 | −1·4 | +3·1 | 14·5 | 14 | 1·26 | 648·7 | ,, | 0 | 111 |
| 67† | Dec. 7 | 7·20–49 | −6·56 | 1·50 | −0·6 | +0·4 | 21·1 | 11·85 | 1·78 | 647·9 | ... | Cirrus 10 | Dull |
| 68† | ,, | 8·10–37 | −6·56 | 0·72 | −0·6 | −1·2 | 26·7 | 11·85 | 2·25 | 648·1 | ... | Str. 10 | ... |

TABLE.—*Observations made in Dry Weather at Ben Nevis High-Level Observatory during Winter*, 1892–93—continued.

| No. | Date. | Hour. | Temperature. | | Change in Weight of | | | Litres of Air Aspirated. | Mgs. of Water Vap. in each Litre of Air. | Pressure, mm. | Wind, 0–12. | Cloud, 0–10. | Dust, per c.c. |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | Dry. | Dry-Wet. | Standard, mgs. | 2nd Tube, mgs. | 1st Tube, mgs. | | | | | | |
| | 1892. | | | | | | | | | | | | |
| 69† | Dec. 22 | 15·51–71 | −2·06 | 0·50 | ... | ... | 44·4 | 12 | 3·70 | 646·4 | S.S.E. 3 | Cir. cum. 8 | 225 |
| 70† | Dec. 23 | 5·20–38 | −3·67 | 0·16 | +0·1 | +0·6 | 27·7 | 8 | 3·46 | 645·7 | S.E. 5 | 0 | 175 |
| 71† | Dec. 24 | 3·47–71 | −4·78 | 3·33 | +0·2 | +0·6 | 10·8 | 12 | 0·90 | 642·8 | S.E. 4 | 0 | 300 |
| 72† | ,, | 5·17–33 | −3·83 | 4·28 | ... | −0·4 | 5·2 | 8 | 0·65 | 642·7 | S.E. 4 | 0 | 175 |
| 73† | ,, | 7·18–34 | −3·89 | 4·17 | ... | −1·0 | 5·9 | 8 | 0·74 | 642·6 | S.E. 4 | 0 | 200 |
| 74† | ,, | 8·49–66 | −4·06 | 4·05 | +0·2 | +0·3 | 6·4 | 8 | 0·80 | 642·9 | S.E. 3 4 | Cir. str. 1 | 440 |
| 75† | ,, | 10·48–73 | −2·84 | 4·60 | ... | +0·6 | 9·6 | 12 | 0·80 | 642·9 | S.S.E. 4 | 0 | 125 |
| 76† | ,, | 13·13–57 | −3·44 | 4·56 | −0·5 | ... | 11·3 | 20 | 0·56 | 642·4 | S.S.E. 3–4 | Cir. str. 8 | 200 |
| 77† | ,, | 14·52–83 | −4·00 | 4·39 | ... | −0·8 | 5·9 | 12 | 0·49 | 642·8 | ... | 0 | ... |
| 78† | ,, | 15·35–68 | −4·00 | 4·50 | ... | −0·1 | 6·0 | 12 | 0·50 | 642·5 | ... | 0 | 412 |
| 79† | Dec. 25 | 4·59–99 | −2·22 | 5·56 | ... | −0·5 | 12·9 | 20 | 0·64 | 641·5 | S. × E. 3 | 0 | 183 |
| 80† | ,, | 7·30–68 | −1·67 | 5·33 | −0·3 | 0·0 | 8·7 | 20 | 0·44 | 642·2 | S. 2–3 | 0 | 105 |
| 81† | ,, | 11· 9–57 | −0·62 | 5·50 | −0·3 | +0·2 | 9·8 | 20 ? | 0·49 | 642·9 | S.E. 0–1 | Cir. 7 | 50 |
| 82† | ,, | 13·42–60 | −1·28 | 5·11 | ... | −0·8 | 8·8 | 16 | 0·55 | 643·3 | S. 1–2 | 0 | ... |
| 83† | ,, | 16· 7–46 | −2·22 | 4·61 | −0·7 | +0·4 | 10·0 | 20 | 0·50 | 643·9 | S.W. 0 | 0 | ... |
| 84† | ,, | 17· 9–51 | −2·22 | 4·66 | ... | −0·1 | 9·9 | 20 | 0·50 | 644·3 | ... | 0 | ... |
| 85† | Dec. 26 | 6·25–66 | −1·22 | 4·67 | ... | +0·2 | 16·8 | 19·9 | 0·84 | 646·3 | ... | 0 | ... |
| 86 | ,, | 11· 8–48 | −1·67 | 4·66 | −0·3 | 0·0 | 15·2 | 23·9 | 0·63 | 647·3 | N.W. 1–2 | 0 | 44 |
| 87 | ,, | 12·12–54 | −1·61 | 4·84 | −0·2 | −0·7 | 14·2 | 23·9 | 0·59 | 647·2 | W.N.W. 1–2 | 0 | ... |
| 88 | ,, | 14·40–75 | −1·17 | 4·83 | −0·6 | +1·3 | 13·6 | 19·9 | 0·68 | 647·2 | ... | 0 | 56 |
| 89 | ,, | 15·23–60 | −1·22 | 4·84 | −1·8 | +2·2 | 13·5 | 19·9 | 0·68 | 647·3 | N.W. 1 | 0 | 72 |
| 90 | Dec. 27 | 2·37–76 | −0·72 | 5·39 | +0·5 | +0·3 | 17·1 | 19·8 | 0·86 | 648·2 | W.N.W. 1–2 | 0 | 50 |
| 91 | ,, | 6·21–58 | −1·28 | 4·66 | +0·5 | +0·4 | 12·3 | 19·8 | 0·62 | 648·5 | W. 1–2 | 0 | ... |
| 92 | ,, | 12· 4–41 | −3·67 | 3·50 | +0·9 | +0·2 | 21·2 | 19·8 | 1·07 | 648·9 | W.S.W. 3 | Cir. 8 | ... |
| 93 | ,, | 12·53–92 | −4·39 | 3·28 | +0·9 | −1·1 | 21·6 | 19·8 | 1·09 | 648·8 | ... | ... | ... |
| 94 | ,, | 15·40–70 | −4·11 | 3·67 | −0·6 | +1·2 | 16·2 | 15·8 | 1·03 | 648·9 | S.W. 3 | Cir. cum. 8 | ... |
| 95 | ,, | 16·20–51 | −4·66 | 3·40 | −0·6 | −0·3 | 16·2 | 15·8 | 1·03 | 648·8 | S.W. 2 3 | Cir. cum. 8 | ... |
| 96 | Dec. 28 | 5·44–68 | −3·06 | 3·33 | ... | +2·5 ? | 19·5 | 11·9 | 1·85 | 647·8 | S.W. 1–3 | Cir. str. 10 | ... |
| | 1893. | | | | | | | | | | | | |
| 97 | Jan. 4 | 10·43–71 | −8·72 | 0·28 | +0·7 | −0·1 | 19·2 | 15·75 | 1·22 | 650·4 | Calm | 0 | 15 |
| 98 | ,, | 11·27–48 | −8·44 | 0·45 | ? | −1·0 | 18·9 | 11·8 | 1·60 | 650·4 | Calm | 0 | 58 |
| 101 | Jan. 11 | 13·45–66 | −1·56 | 3·22 | −1·0 | −0·7 | 20·7 | 12·0 | 1·72 | 654·2 | ... | ... | ... |

NOTES.—In all experiments, except those marked †, the standard tube was used as a counterpoise. The following experiments are not given in the above Table :—Nos. 1–4 were test experiments made at the Low-Level Observatory, and 99 inside tower at High-Level Station. Nos. 5, 7–18, 37–42, 50, 51 were made in saturated or supersaturated air. Nos. 27, 28, 44, 52, 53, 56, 60, 100, the drying-tube was found broken when the box was taken in after the experiment, or the amount of air aspirated was not exactly determined owing to leakage.

calculating vapour pressure, relative humidity, and dew-point from dry and wet bulb observations agree very badly—indeed, at times disagree very remarkably. I have reduced some of my observations by means of Guyot's Smithsonian Tables (last edition) as well as those of Mr Glaisher. Here are examples of the differences that may occur (the temperatures are expressed in degrees Fahrenheit):—

| Dry. | Wet. | Difference. | Vapour Pressure. | | Relative Humidity. | | Dew-point. | |
|---|---|---|---|---|---|---|---|---|
| | | | Glaisher. | Smithsonian. | Glaisher. | Smithsonian. | Glaisher. | Smithsonian. |
| 20·2 | 17·3 | 2·9 | ·037 | ·062 | 34 | 55 | − 3·3 | 8·0 |
| 20·3 | 19·4 | 0·9 | ·078 | ·094 | 73 | 87 | 13·1 | 17·0 |
| 36·8 | 30·0 | 6·8 | ·109 | ·084 | 50 | 41 | 20·3 | 14·5 |
| 42·7 | 36·5 | 6 2 | ·160 | ·135 | 59 | 49 | 29·1 | 25·0 |

Considering the few experiments made when testing the apparatus at low levels, the results appear to agree fairly well with those of Glaisher's Tables, but the number of these is not sufficient to warrant any definite statement.

The need for additional experiments is obvious. Results at summer temperatures are wanted to complete the diagram; and, to determine how far pressure influences the other factors, a series of simultaneous experiments must be made at two extreme stations at least, and, if possible, also at an intermediate one. To ensure perfect comparability of data, special precautions must be taken, for instance, by having the dry and wet bulbs always in analogous conditions by using some aspiration psychrometer, and it would be well at the same time to record the readings of other instruments used for determining the hygrometric conditions of the atmosphere.

In conclusion, I would thank the Directors of the Ben Nevis Observatory for giving every facility for carrying out these experiments, and to the observatory staff, especially to Messrs Omond and Rankin, for their help, without which the number of the experiments would have been much less. I am also especially indebted to Professor Tait and Dr Buchan, who have aided me in every way in planning and discussing the results of this research; and I thank Mr James Wood for helping me with the calculations.

# The Second and Fourth Digits in the Horse: their Development and Subsequent Degeneration. By J. C. Ewart, M.D., F.R.S., Regius Professor of Natural History, University of Edinburgh.

*Preliminary.*

(Read March 5, 1894.)

During the last fifty years not a few familiar and apparently quite uninteresting structures have all at once assumed unusual importance and arrested the attention of naturalists in all parts of the world. This is especially true of what used to be known as rudiments, of what we nowadays generally designate vestiges. Of all the known vestiges, the most familiar are perhaps the "splint" bones of the horse. For long the interest in these splints was extremely limited, but since it was shown that they correspond to the functional second and fourth metacarpals and metatarsals of other vertebrates they have attracted the notice of both comparative anatomists and palæozoologists, and are now invariably looked upon as affording strong evidence in support of the view that the horse has descended from polydactylous ancestors.

Since, through the work of Gaudry, Marsh, Cope, and others, it became possible to construct a wonderfully complete pedigree for the horse family, and especially since Huxley, in his address on "Fossil Horses," gave a graphic account of the ancestors of recent horses, many have wished that, by way of making the relationship with Hipparion still more certain, "rudiments" of the phalanges of the second and fourth digits could be shown to persist at the ends of the "splints." Gegenbaur once suggested that there might be latent "germs" of the lost digits, but apparently no one has hitherto discovered the "germs" or rudiments in the embryo or found the vestiges in the adult. This being the case, it may seem strange that I am now in a position to assert that it is all but impossible to study the development of the horse without observing the development and subsequent degeneration of the second and fourth fingers and toes, or to examine the adult skeleton without seeing their vestiges.

To throw, if possible, some light on polydactyly in the horse, I was recently led to study the development of the skeleton of the limbs. One result has been the discovery of the long-missing phalanges of the second and fourth digits. Their rudiments have been recognised and their development and subsequent degeneration followed until they lose their identity and become inseparably connected with their respective metacarpals and metatarsals, to form what are familiarly known as the "buttons" of the "splints."

It seems to be generally admitted that one of the first steps in the formation of the joints of the digits consists in the division of the primitive rods of cartilage into segments by discs of embryonic cells. The segments eventually give rise to the phalanges, while the cellular discs seem to be especially concerned in the formation of the various parts of the joint, *e.g.*, the articular cartilages, the capsule, and the membrane lining the joint cavity.

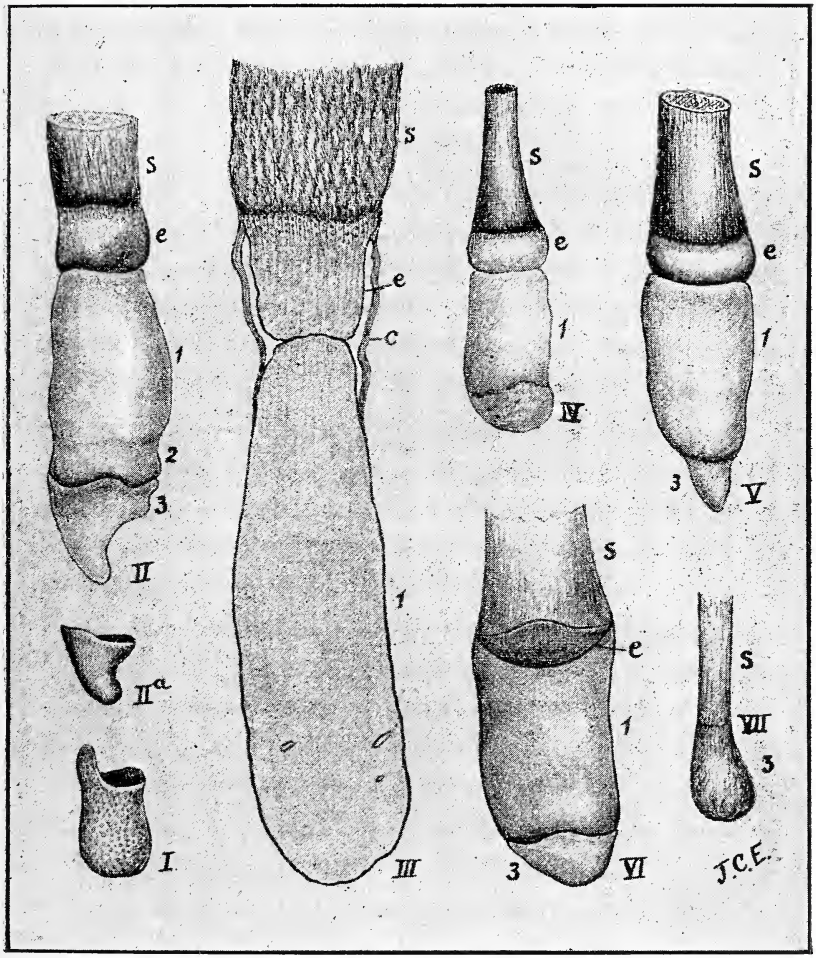
I have not hitherto succeeded in finding distinct articular discs in the second and fourth digits of the horse, even in very small embryos; but I have observed in some cases clear lines running across the rods of cartilage in the position of the joints. It is possible that, owing to abbreviation (or to arrest) in development, true discs are never formed in these useless digits. Though clear evidence of phalanges was not found in a 20 mm. embryo, I made out easily enough the position of the metacarpo-phalangeal joint in a 25 mm. embryo. In this embryo there was no indication of a joint cavity or capsule, but a very thin layer of cells * separated a terminal nodule (representing the phalanges) from the metacarpal proper.

This terminal portion (fig. I.), when removed, was seen to present at its proximal end a slightly concave surface. The distal end of the metacarpal was smooth and slightly rounded, and composed of apparently normal cartilage cells.

The cartilage forming this nodule was found, on further examination, to be so invested with cells and fibres that it was impossible to ascertain whether either of the phalangeal joints had been developed. Partly because of its minute size, and partly because of its interest, I did not care to sacrifice it to the microtome.

* These cells probably represent an articular disc.

Vestigial structures are generally supposed to reach their maximum development early, and then to either degenerate or disappear. From the material already examined, it seems the second and fourth digits reach their highest development when the embryo




is from 30 cm. to 40 cm. in length. For example, in an embryo 35 cm. in length the second and fourth digits were remarkably complete. Including the metacarpals, they varied in length from 3·2 to 3·5 cm.—the phalangeal portions varying from 3 to 4 mm.

Each metacarpal and metatarsal consisted of an ossified shaft and of cartilaginous extremities; the proximal piece of cartilage measured from 4 to 5 mm., the distal from $1\frac{1}{2}$ to 2 mm. The phalangeal part consisted of an elongated piece of cartilage representing the first and second phalanges (1, 2, Fig. II.), and of a small terminal portion representing the third phalanx (3, Fig. II.). The large piece was articulated to the slightly-rounded end of the epiphysis by an imperfectly-formed joint. The joint between the second and third phalanges looked as complete as the basal joint, but, as sections showed, this was not the case. In some sections the epiphysis of the metacarpal was completely separated by a distinct gap from the first phalanx, but, in the case of the second and third phalanges, though there was a deep cleft extending well across between them, the separation was never complete. The formation of a joint between the first and second phalanges in the specimens examined had either never been attempted or it had been arrested at the initial stages. At the most, there was a shallow cleft on what appeared to be the anterior or extensor aspect.*

Having once recognised that there is a piece of cartilage beyond, and quite distinct from, the epiphysis at the distal end of the metacarpals and metatarsals, there is never any difficulty in making out the metacarpo-phalangeal and metatarso-phalangeal joints. But in most cases it is extremely difficult to define the phalanges, more

* The practical absence of a joint between the first and second phalanges is extremely interesting. Granting that the effects of disuse are not transmitted, it is all but impossible to account for the gradual reduction of digits generation after generation, century after century. Anything that throws light on the *modus operandi* of reduction is therefore well worth recording. In man the little toe is slowly changing from a three-jointed to a two-jointed toe. This degeneration has often been ascribed to boot-pressure, and has been again and again brought forward as an example of the transmission of acquired characters. The condition of the second and fourth digits in the embryo horse, considered along with the fusion of the first and second phalanges of a "restored" second digit of a foal in my collection, may be held to prove that degeneration of the digits is, apart from any external influence, accompanied by an arrest in the formation of the phalangeal joints. External pressure could not, of course, be a factor in the degenerative process in the case of the horse. Hence the reduction of a digit may be said to consist not only of an arrest in the growth of the phalanges but also of an arrest in the formation of the joints between them—not, as might have been expected, by the disappearance, one after another, of the phalanges from below upwards. Why this arrest should take place in some digits and not in others, and proceed at an increasing rate generation after generation, is, as already indicated, extremely difficult to explain.

especially the terminal phalanx. This is due to the fact that the vestigial fingers and toes are encased (wrapped up like mummies) in several layers of extremely dense connective tissue. In the 35 cm. embryo I only succeeded in completely unwrapping the second digit of the right manus, after prolonged maceration in cedar-wood oil.* When the investing structures were removed this digit had the appearance shown in figure II. The terminal phalanx was curved inwards, and was almost an exact miniature of the corresponding phalanx in the manus of the polydactylous foal's foot already referred to. Even more remarkable than the shape of the phalanx is the fact that its apex was encased in a small cellular cap (fig. II.*a*). This cap, which was easily detached, is difficult to account for. It may represent the bony cap which I recently found on the large middle digit of the horse,† or it may correspond to one or more of the deeper layers of the hoof.

The second digit of the manus is the one most frequently found in polydactylous horses. This being the case, it is worth noting that the phalanges of the second digit of the manus in normally developing horses appear to be always larger and better developed than those of the fourth, and also that the vestigial digits of the manus seem to be better developed than those of the pes.

In no instance have I found the second and fourth digits better developed than in the 35 cm. embryo. In most of the older embryos examined marked degeneration had already set in.

In some cases, up to within two months of birth, they, though much larger, closely resembled the digits in the 35 cm. embryo. In other cases the joint between the second and third phalanges early disappeared, with the result that each of the second and fourth fingers and toes were represented by single elongated pieces of cartilage connected by a fairly well-formed joint to the epiphysis at the end of their respective metacarpals and metatarsals. Hence it may be inferred that the second stage in the retrogressive process consists in the complete disappearance of all indication of a joint between the first and second phalanges and the all but complete disappearance of the joint between the second and third phalanges.

* I may mention that on removing the investing tissues I noticed what looked extremely like vestiges of the flexor tendons.

† Ewart, *Jour. of Anat. and Phys.*, January 1894.

The smallest embryo in which all three phalanges were more or less completely fused was 60 cm. in length. Figure III. represents a longitudinal section through the distal portion of the second digit of the pes. This figure is an extremely important one. I have considered very anxiously and fully all the possible objections to my interpretation of the facts established, and, as far as I can see, the only possible criticism is that the cartilages I look upon as representing phalanges are only epiphyses. The section figured fully disposes of this criticism. It shows the ossified distal end of the shaft (*s*) of the second metatarsal supporting a cartilaginous epiphysis (*e*), which, when examined with a high power, presented all the characteristic appearances in a typical epiphysis. If this cartilage (*e*) is the epiphysis of the metatarsal, the piece of cartilage (1) beyond must be something else. The piece of cartilage (1) cannot be an epiphysis, for an epiphysis is never articulated to its shaft; it must, in fact, represent one or more of the phalanges of the second digit. Convincing proof of this is afforded by the presence of the joint at the end of the epiphysis. Dr Hepburn, who has made a special study of joints,* on seeing the specimen from which the drawing was made, without knowing anything of its origin, at once stated that it showed an arrested diarthrodial joint, *i.e.*, a joint which in an imperfect way resembled the joints of our fingers and toes. When first dissected there was free movement at the metatarso-phalangeal joint represented in the section, and when the capsule of the joint at the end of the fourth metatarsal was opened a droplet of fluid was seen to escape. That this is possible will be at once evident if the extent of the cavity of the capsule is taken into account. Hence it may be held as proved that, up to at least the sixth month of fœtal life, the metacarpo-phalangeal and metatarso-phalangeal joints, though not well formed, are quite distinct and functional. Figure IV. represents the fourth digit of the manus in an eight-months' fœtus, figure V. the same digit in a nine-months' fœtus, while figure VI. shows the condition of the second digit one month after birth.

In figure IV. the epiphysis is now much shorter, and the shaft of the metacarpal (II.) is expanded at its lower end, and there is an indication of where the terminal phalangeal joint originally existed.

* *Journ. of Anat. and Physiol.*, vol. xxiii. p. 507.

In the specimen represented in figure V. the metacarpo-phalangeal joint and the capsule were well developed, and the tip of the digit (perhaps the relatively small third phalanx) was pointed and slightly movable. The epiphysis of the metatarsal (II.) was still cartilaginous. Figure VI. shows the distal end of the now fully-developed second metacarpal. The distal epiphysis, which is now very short, is ossified and fused to the shaft—only a faint streak indicating the line of junction. The free end of the epiphysis is rounded and completely grasped by the now concave proximal end of the first phalanx. The cavity of the metacarpo-phalangeal joint had completely disappeared. I was not a little surprised to find in the one-month foal a separate terminal phalanx freely movable on the now greatly altered piece of cartilage representing the first and second phalanges. The fourth digit of the same manus was smaller and all the phalanges had completely blended.

To complete the "buttons," all that is now necessary is that the united phalanges should undergo ossification, and afterwards fuse with their respective metacarpals and metatarsals. A fully developed "button" is represented on fig. VII. When the ossification sets in, how many centres appear, and when the last trace of the presence of phalanges is obliterated, I have not yet been able to determine, but, in all probability, early in the second year the "buttons" completely coalesce with their "splints."

Having shown that the second and fourth digits make their appearance during the development of the horse, another link has been added to the chain of evidence in favour of the horses of to-day being closely related to the three-toed horses of the Pikermi and other Miocene deposits, and remotely related to the primitive polydactylous horse-like forms of the Lower Eocene. It may, in fact, be said that the last link in the chain of evidence has at last been forged, and that there is now less than ever any escape from the conclusion that the horse has descended from polydactylous ancestors. Further, the presence of "rudiments" of the second and fourth fingers and toes in the embryo may help us in accounting for the occasional presence of extra digits in fully-developed horses.

## Note on the Focus of Concavo-Convex Lenses the Surfaces of which are of Equal Curvature. By George A. Berry, M.B., F.R.C.S. Ed.

(Read December 18, 1893.)

The ophthalmic surgeon has frequently to prescribe convex lenses of less than four inches focus for the correction of the optical defect in the eye left by removal of the intransparent crystalline lens in the operation for cataract. Occasionally, too, equally strong concave lenses are required to correct high degrees of short sight.

Sometimes it is necessary to ascertain if the optician has ground a particular lens accurately according to prescription. The readiest method of doing this is to take a lens of opposite sign to, and same focus as, that prescribed, and observe whether or not, when the lenses are placed in contact, they neutralise each other.

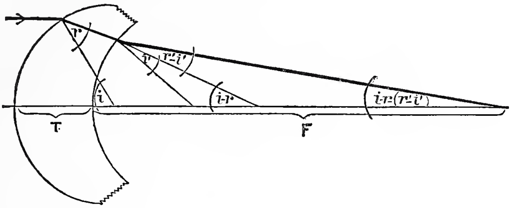
In the case of weak lenses (of more than four inches focus), a practical neutralisation is got when plus and minus glasses of equal strength are combined. For strong convex lenses, however, a stronger concave, and consequently for strong concave a weaker convex lens is required to produce the effect of neutralisation.

The positive effect given by the combination of equally strong concave and convex lenses is more marked, and therefore appreciable with weaker lenses, on putting the concave surface next the eye, than when the combined lenses are held in the opposite way.

The two lenses, plus and minus, of equal strength when held together so that one surface of each lens are in contact, are equivalent to a lens the two surfaces of which are of equal curvature, but one convex and the other concave.

The properties of such a lens, inasmuch no doubt as it probably cannot be put to any use for which other forms of positive lenses are not more suitable, do not seem to have been studied.

From the accompanying figures it is seen that F, or the distance along the axis (measured from the other surface) at which a ray parallel to the axis before refraction meets it after refraction, has the following values according as the ray impinges on the convex or concave surface first:—




From fig. 1—

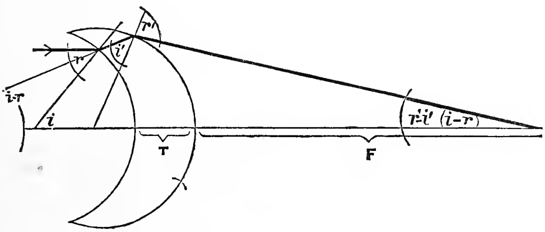
$$F = \frac{R \sin r'}{\sin\{(i-r)-(r'-i')\}} + R ,$$

where R is the radius of both surfaces, and $i$, $i'$ and $r$, $r'$ the angles of incidence and refraction at the two surfaces. The relation between these angles is given by

$$\sin i = \mu \sin r ,$$
$$\sin r' = \mu \sin i' ,$$

and

$$\sin i' = \sin r - \frac{t}{R} \sin (i-r) .$$




From fig. 2—

$$F = \frac{R \sin r'}{\sin\{(r'-i')-(i-r)\}} ,$$

and

$$\sin i' = \sin r + \frac{t}{R} \sin (i-r) .$$

For axial rays these general values become respectively

$$F = \frac{R\{R\mu - t(\mu - 1)\}}{t(\mu - 1)^2} \quad . \quad . \quad . \quad (\alpha)$$

and

$$F = \frac{R\{R\mu + t(\mu - 1)\}}{t(\mu - 1)^2} \quad . \quad . \quad . \quad (\beta)$$

The difference between these two values of the principal focal distances (measured from the opposite surface) is evidently

$$\frac{2R}{\mu - 1}.$$

(For $\mu = 1{\cdot}5$ the difference is 4R.)

The principal points (as can be shown to be the case in other ways) must therefore lie to the side of the convex surface at distances respectively of $\frac{R}{\mu - 1}$ from each surface and separated by a distance $= t$.

If the principal focus be measured from the principal points, we have to add or subtract $\frac{R}{\mu - 1}$ from the above values.

It then becomes in both cases

$$F = \frac{R^2}{t} \cdot \frac{\mu}{(\mu - 1)^2} \quad . \quad . \quad . \quad . \quad . \quad (\gamma)$$

which shows that *the focus of such a lens increases directly as the square of the radius of either surface, and inversely as the thickness.*

($\gamma$) may be written

$$F = \frac{1}{t} \cdot \frac{\mu R}{\mu - 1} \cdot \frac{R}{\mu - 1},$$

by which its relation to the formula for refraction at one surface is shown.

$$\left(\text{For } \mu = 1{\cdot}5 \ \gamma \text{ becomes } F = \frac{6R^2}{t}\right).$$

The difference between ($\alpha$) and ($\beta$), or, in other words, the position of the principal points, accounts for the different degree of neutralisation produced according as one side or other of a combined convex and concave lens of equal focus is held to the eye.

From the formula $\frac{6R^2}{t}$ (in which $\mu = 1 \cdot 5$) we see that by making $t = 3R$, $F = 2R$.

These measurements have been given to this lens (shown), which is therefore focussed when in contact with the object looked at if the convex surface be next the eye, but has to be withdrawn to 4/3 its thickness if looked through in the opposite direction. This is shown as illustrating, in the most exaggerated manner, the difference in the focus as measured from each surface.

The great spherical aberration of a lens whose surfaces are of equal curvature must render it of little or no practical use. Possibly, under certain conditions, such a lens might be used to counteract the aberration of a much stronger concave lens.

If two plano-cylindrical lenses (of the kind in general use for the correction of simple astigmatism) of equal strength and opposite signs be combined so that their axes coincide while their plane surfaces are in contact, the result is, of course, a lens having the effect of a much weaker convex cylinder. Owing to greater spherical aberration, a cylindrical lens of this kind produces a greater astigmatic effect than an ordinary plano-cylindrical lens of the same strength. For instance, by the combination of convex and concave plano-cylinders of 20 cm. focus and thickness 5 mm., the focus according to the above formula is 12 metres. Yet such a lens causes a greater astigmatic effect than a plano-cylinder of 4 metres focus.

The two concavo-convex cylindrical lenses shown have respectively for each surface a radius of 5 cm. with thickness 7·15 mm., and therefore focus 2·3 metres, and a radius of 10 cm., thickness 4·45 mm., and focus 13·5 metres.

From recent observations with Javal's ophthalmometer, it has been shown that in many cases of corneal astigmatism the degree of astigmatism at different parts of the cornea is far from being the same. It seems just possible that in some cases of this kind a better correction might be got by means of a concavo-convex cylinder, so that in this direction there might be a practical application.

## Telegraphic Communication by Induction by Means of Coils. By Charles A. Stevenson, B.Sc., F.R.S.E., M.Inst.C.E. (With Two Plates.)

(Read March 19, 1894.)

In 1892, I suggested that communication could be established between ship and ship by means of coils,* and as a trial of the system on a large scale has recently been made with the view of establishing communication between North Unst lighthouse, situated on Muckle Flugga, and the mainland (fig. 1), thence to the lighthouse station at Burrafiord, a distance of two miles, a record of the trials may be of interest to the Society.

The induction of one spiral on another has been long known, but with a very strong battery current it has been found impossible to bridge a greater distance than 100 yards,† so that as a means of practical communication it was impossible. It has also been long known that communication could be established by means of parallel wires, and disturbances in wires no less than ten miles apart had been detected. For many years this system has been under discussion, and only last month a series of elaborate experiments at Loch Ness has been made by Mr Preece on this parallel wire system on the most approved methods; but I trust to be able to show that the parallel wire system ‡ must give place to the method of communicating by coils.

It is evident that if two coils are placed so that their axes are coincident, their planes being parallel, or if they be placed so that their planes are in the same plane, they will be in good positions to expect electric currents sent in one to be apparent by induction in the other. For a given diameter, and where the electrical energy is small and the number of turns small, the first position is best, but where the energy is great and the number of turns great—in fact, when it is wished to carry the induction to many times the diameter of the coils—then it will be found that it is better to let the two coils be in the same plane, as, when the axes are coincident and the coilsa great distance apart in comparison with the diameter, the difference of distance from one side of the

* *Engineer*, vol. lxxiii. p. 292. † *Jour. of Society of Arts*, vol. xlii. p. 274.
‡ *British Ass. Reports*, 1886, p. 546; 1887, p. 611.

coil, say top of primary coil to top and bottom of secondary, becomes almost a vanishing quantity; whereas, when the coils are lying on their side in the same plane, the difference of distance from back of primary to back of secondary, and from front of primary to front of secondary, does not fall off so fast, and consequently is more efficacious. Besides, it becomes impracticable to erect coils of large diameter with their planes vertical, but it is easy to lay them on their sides. It is also impracticable to introduce a core in these large coils, although the effect would thereby be intensified, and where compactness is necessary a core is advantageous.

A number of experiments were made in the laboratory to discover the laws of the action of coils on each other, with a view of calculating the number of wires, the diameter of coils, the number of ampères, and the resistance of the coils that would be necessary to communicate with Muckle Flugga, and, after a careful investigation, it was evident that the gap of 800 yards could, with certainty, be bridged by a current of one ampère with coils of nine turns of No. 8 iron wire in each coil, the coils being 200 yards in diameter.

Two coils, about 850 yards centre to centre, were erected at Murrayfield (and I may here thank Mr Gibson, telegraph engineer of the General Post Office, and his staff, and also Mr Asher of the National Telephone Company, and Messrs Clement & Francis of the North British Railway, for their valuable assistance), one coil being on the farm of Damhead, and the other on the farm of Saughton, and as nearly as was possible on a similar scale, and the coils of similar shape,* as was wished at Muckle Flugga.

On erecting the coils, communication was found impossible, owing to the induction currents from the lines from Edinburgh to Glasgow, the messages in these lines being quite easily read, although the coils were entirely insulated and were not earthed. The phonopore which the North British Railway Company have on their lines kept up a nearly constant musical sound, which entirely prevented observations. On getting the phonopore stopped, it was found that 100 dry cells, with 1·2 ohms resistance each and 1·4 volts, gave good results, the observations being read with great ease in the secondary by means of two telephones. The cells were reduced in number

* Shape of coil, circle, square, or rectangle, &c., is not very material, and in practice must be altered to suit circumstances, as for instance in a ship (fig. 2).

down to 15, and messages could still easily be sent, the resistance of the primary being 24 ohms, and the secondary no less than 260 ohms. If the circuit had been of good iron, with soldered joints, and well earthed, the resistance should have been only 60 ohms. The induced current therefore generated in the secondary would therefore be in the ratio of 480 to 210, or with this great resistance, allowing for the resistance in the two telephones in multiple, we got practically only half the current we would have got if the line had been a permanent in place of a temporary one.

A trial was made of the parallel wire system, and with 20 cells the sound was not heard, and with 100 cells it was heard by me as a mere scratch in comparison with the sound with the coil system with 15 cells. A trial was made with a phonopore, and the coils worked with 10 cells with perfect ease, and a message was received with only 5 cells. Speech by means of Deckert's transmitter was just possible, but it is believed that if the hearing circuit had been of less resistance it would have been easy to hear.

It is difficult to understand how this system of coils, in opposition to the parallel wire system, has not been recognised as the best. For, assume that with the arrangements we had, we heard equally well with 100 cells by both systems, both having the same base (200 yards), then by simply doubling the number of turns of wire, and using thick wire of low resistance, the effect would have been practically doubled, whereas, by the parallel wire system, there is nothing for it but to increase the battery power, which, *for practical working, becomes an impossibility.* The difficulty of the current is thus removed by using a number of turns of wire. There is shown on the following diagram (fig. 3) the result of simply increasing the diameter with a given length of wire, keeping current and resistance the same. It shows that the larger the diameter the better; in fact, with a given length of wire, a straight one is the best. But this is not practicable; what is wanted is to get induction at a great distance from a certain given base with a small battery power, and the laboratory experiments and the trials in the field show that the way to overcome the difficulty of the current is by using a number of turns of wire The secret of success is to apportion the resistance of primary and secondary and the number of turns on each to a practical battery power.

*Coil System.*—(1) At 850 yards from centre to centre of coils averaging each 200 yards diameter, it was found that, with a phonopore, messages were sent with 5 dry cells. The resistance in primary being 30 ohms, and the resistance of secondary 260 ohms, the current being ·23 ampère, which, with 9 turns, gives 2 ampère turns. (2) With a file as a make and break it worked with 10 cells, giving ·4 ampère or 3·6 ampère turns.

*Parallel Wire System.*—(3) With a file as a make and break, and with parallel lines earthed, it was heard with 100 cells, giving 1·1 ampère. Fig. 4 shows the variation with number of turns on one coil, the current and resistance remaining constant.

The calculation of the diameter necessary to hear a given distance is simple, from the fact that the hearing distance is proportional to the $\surd$ of the diameter of one of the coils, or directly as the diameter of the two coils, so that, with any given number of ampères and number of turns to hear double the distance requires double the diameter of coils, or double the number of turns, and so on. But this is within certain limits, for when the coils are close to one another the law does not hold.

There is one point which seems to have been cleared up by these trials, which has even this month been a subject of discussion in London, and that is, whether or not the parallel wire system is actuated by induction or conduction, and there is little room for doubt, from the fact that both circuits were insulated, and was thus a case of pure induction, that to a large extent it is induction; in fact, that they act together, and it will depend how the ends are earthed, or, in short, what is the distance bridged in comparison to the breadth of base, which predominates. Where the wires are long in comparison with the distance bridged, conduction will be the main working factor, but when the base is small, and the distance bridged is large in comparison, induction will be the main factor, and the number of turns then increases the effect.

The primary coil was insulated in the Murrayfield trials, as at Muckle Flugga it must be so, the impracticability of making and maintaining the sea connections necessitating this, and the secondary was earthed, as is most convenient at Muckle Flugga. When the secondary was also made a complete insulated metallic circuit, there seemed to be little difference in the result.

There is one other point to which reference must be made. Mr Preece has been repeating the experiments brought before this

Society on 30th January 1893, and he found, if rightly reported, that when the hearing wire was floating he got results, but when it was allowed to descend that no observations could be got; he attributes this to reflection from the surface of the water; this, however, is unlikely, as the reason that no sounds were then heard was that the major part of the wire lay on an equipotential line. Electro-magnetic waves enter or leave salt water practically unimpaired, as the following diagram (fig. 5) will show, where A is a wire suspended 10 feet above salt water and B is a wire sunk in salt water. On trial, it was found, as stated in my paper read before this Society in January 1893, that there was no *practical* difference in air or salt water to the propagation of electro-magnetic waves, in or out the distance to which waves went, *i.e.*, the distance to which the currents could be heard being immaterial whether the detector was sunk or in air.

It has been attempted to be shown that the coil system is not only theoretically but practically the best. Meantime, my brother has recommended the Commissioners of Northern Lighthouses to erect the coil system at Muckle Flugga, and the Commissioners have approved, and I hope that we may soon hear of this novel system of communication being erected at the most northern point of the British Isles, where the laying down and after-maintenance of a submarine cable is practically impossible. This system is also applicable to our warships, to assist in their manœuvring, by the establishment of instantaneous communication unaffected by wind or weather.

The application of the coil system to communication with light-vessels is obvious, namely, to moor the vessel in the ordinary way, and lay out from the shore a cable, and circle the area over which the lightship moorings will permit her to travel by a coil of the cable the required diameter, which will be twice the length of her chain cable. On board the vessel there will be another coil of a number of turns of thick wire. Ten cells on the lightship and ten on the shore will be sufficient for the installation. The system erected at Kentish Knock and other light-vessels is expensive in moorings, and is liable to derangement, and requires special appliances; whereas, by the coil system there can be no derangement, and the vessel can be moored in the ordinary way. A call arrangement and telephones complete the installation.

# Mr. C. A. Stevenson on Telegraphic Communication by Induction by means of Coils.


Fig. I

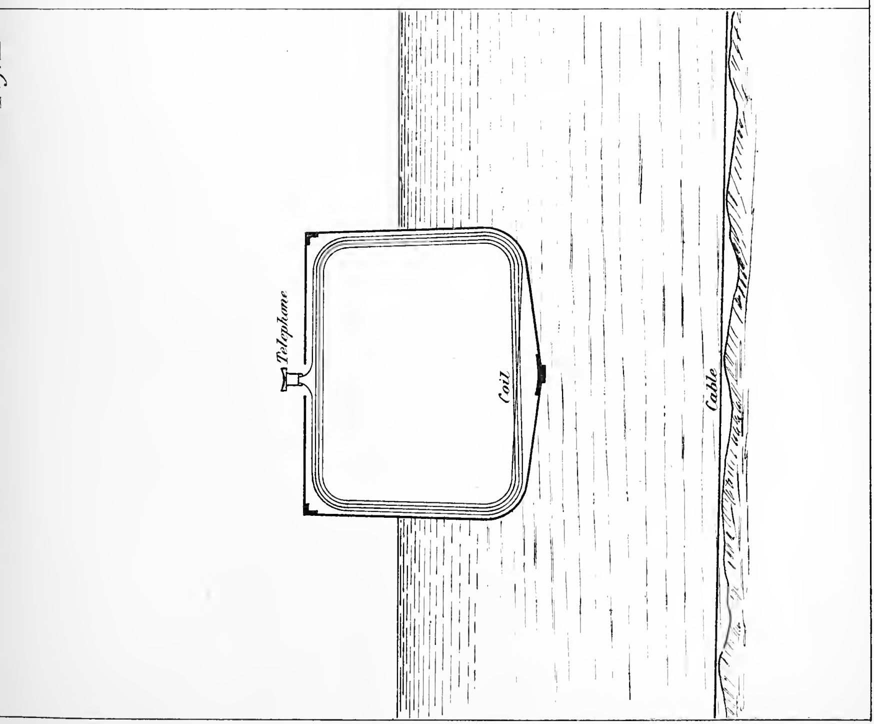



Fig. II

A Ritchie & Son, Edin^r

# Mr. C. A. Stevenson on Telegraphic Communication by Induction by means of Coils.

*Fig. V.*

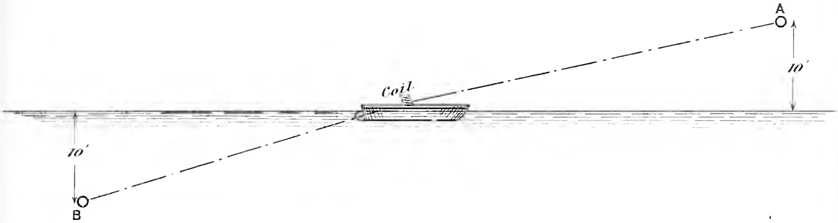



*Fig. IV.*

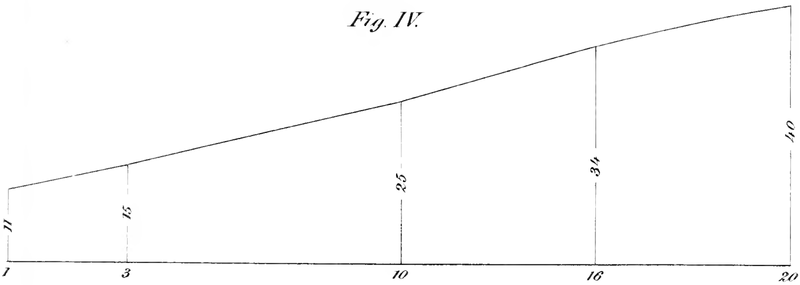
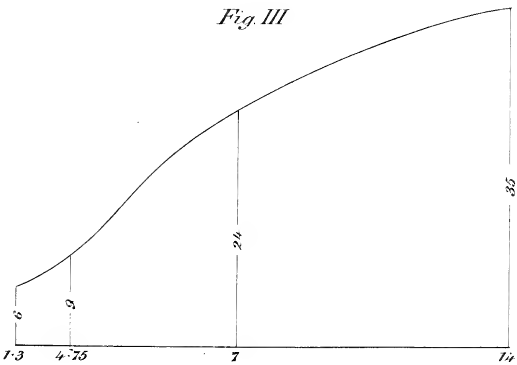



*Fig. III*




A. Ritchie & Son Edin^r.

## On Two Stereo-Isomeric Hydrazones of Benzoin. By Alexander Smith, B.Sc., Ph.D.

(Read January 15, 1894.)

While the theory of stereo-isomerism has been employed in the case of many carbon compounds, it has had but few applications in that of organic substances containing nitrogen. Its use has been almost confined to explaining the occurrence of isomers among the oximes. The addition of a new member to the small list of four or five pairs of hydrazones, whose isomerism can be explained only on the assumption of diversity in the disposition of the atoms in space, is therefore not unwelcome as giving additional weight to the theory.

Werner (Berichte 23, 2333) has prepared the two stereo-isomeric ketoximes of benzoin, and, in accordance with Hantsch and Werner's theory, assigns to them the formulæ :—

$$\begin{array}{rl} C_6H_5 - & C - CH(OH) - C_6H_5 \\ & \| \\ HO - & N \end{array} \qquad \begin{array}{ll} C_6H_5 - C - CH(OH) - C_6H_5 \\ \quad\quad\;\; \| \\ \quad\quad\;\; N - OH \end{array}$$

| α-Benzoin-oxime. | β-Benzoin-oxime. |
|---|---|
| Stable, M.P. 151°. | Unstable, M.P. 90°. |

The two phenyl-hydrazones described below will therefore possess the formulæ :—

$$\begin{array}{rl} C_6H_5 - & C - CH(OH) - C_6H_5 \\ & \| \\ C_6H_5HN - & N \end{array} \qquad \begin{array}{ll} C_6H_5 - C - CH(OH) - C_6H_5 \\ \quad\quad\;\; \| \\ \quad\quad\;\; N - NHC_6H_5 \end{array}$$

| α-Hydrazone. | β-Hydrazone. |
|---|---|
| Stable, M.P. 158–159°. | Unstable, M.P. 106°. |

The following four paragraphs give briefly the reasons for believing that the substances isolated are really physical isomers.

1. When benzoin and phenyl-hydrazine, in molecular proportions, are dissolved in alcohol and warmed on the water-bath for four hours, the solution deposits on cooling, first a substance melt-

ing at 106°, and later another melting at 158–159°. They are both found to possess the composition of a monophenyl-hydrazone of benzoin, and to be identical in molecular weight. These facts, with the similarity in mode of preparation, point to identity in chemical constitution.

The lower-melting isomer may be made alone by heating the constituents together without intervention of alcohol.

2. Both substances form small white needles. The differences between them are differences in degree only. The melting-points are 52–53° apart. The lower-melting isomer here, as in previously described cases, is much more soluble in ordinary solvents than the other, and is much less stable in its general relations. Following, therefore, the established precedent, it has been designated the $\beta$-hydrazone, to distinguish it from the more stable, or $\alpha$-hydrazone. The relative instability of the $\beta$ variety is shown by the fact that it is rendered oily by heating in alcohol for some hours, and is soon so far decomposed that no crystals can be obtained from the solution. The $\alpha$ variety is unaffected by this treatment.

3. The identity in chemical constitution was proved by the conversion of both into the already known diphenyl-hydrazone of benzil, $C_6H_5 - C(N_2HC_6H_5) - C(N_2HC_6H_5) - C_6H_5$, by the action of excess of phenyl-hydrazine in acetic acid solution, and by other reactions.

4. Finally, as in the case of other isomeric hydrazones, the $\beta$ modification was transformed into the $\alpha$ modification. This was accomplished by heating it in alcoholic solution with two molecules of phenyl-hydrazine. When we consider the instability of the $\beta$-hydrazone, the yield of ·5 gram of the pure $\alpha$-phenyl-hydrazone from 1 gram of the isomer indicated an unexpectedly complete transformation.

This case of stereo-isomerism is specially interesting, as in no previous case could both modifications of the hydrazone be obtained directly. One of the isomers was always obtainable only from the ketone-chloride, or by transformation from the other.

## On Certain Electrical Properties of Iron Occluding Gases.

By **S. Kimura**, *Rigakushi.* (With Two Plates.)

(Read December 18, 1893.)

### I. *The Driving-off of the Occluded Gases by Heating.*

Since the occlusion of hydrogen and carbonic oxide by palladium and iron was first discovered by Graham, many similar effects have been discovered. Prof. Knott * has investigated in considerable detail the very peculiar changes in the thermo-electric position and in the electrical resistance of palladium when variously charged with hydrogen. With respect to iron, Dr Monckman † has experimented on the resistance of iron occluding carbonic oxide, but, in regard to hydrogen, he came to the conclusion that no change in the thermo-electric properties could be detected.

My object in making the following experiments was at first to investigate the thermo-electric properties of iron occluding various gases, and, on pursuing the experiments, I observed in the course of heating a very peculiar change, substantially the same as was observed by Prof. Knott on hydrogenium. The iron used in these experiments was a sample of commercial wire, and its thermo-electric power was numerically indefinite; but, besides this, the thermo-electric change itself was smaller than in the case of two severed halves of the same specimen of wire, which had been annealed with different degrees of care. I therefore changed the subject of investigation, and thereafter paid exclusive attention to the escape of the occluded gases.

First, I took an iron wire occluding carbonic oxide, and obtained a fairly good result. Then the existence of carbonic acid in meteoric iron suggested me to try this gas also. In this experiment I obtained a result equally well defined with that observed in the case of carbonic oxide. Lastly, I tried iron occluding hydrogen. As to

* *Proc. Roy. Soc. Edin.*, 1882–83; *Trans. Roy. Soc. Edin.*, 1886.
† *Proc. Roy. Soc. Lond.*, 1888.

copper occluding hydrogen, the effect was too small to be observed, probably because of the small quantity of the gas occluded, as well as the inadequacy of the process adopted.

The iron wire I used was 0·94 mm. in diameter, and in length about 120 cm. After being straightened by annealing, it was put into an air-tight glass tube of diameter about 3 cm., the air being pumped off from it and replaced by carbonic oxide or carbonic acid gas. I then passed a strong dynamo current through the wire to make it yellow-hot for a few minutes, and allowed it to cool in these gases. At the same time, the passage between the gas-holder and the tube was left open, through a drying-tube. To make the hydrogenised iron, I employed the electrolytic process; and to get the distribution of hydrogen as uniform as possible, the wire was loosely wound round a thin circular board of wood and plunged into acidulated water. The platinum electrode was placed nearly over the centre of the board. The wire which occluded either of the compound gases showed a light-blue, crystalline, lustreless, fibrous surface, not different from that of wire which had had a dynamo current passed through it and had been allowed to cool in the air. In these cases the surface of the wire became very brittle and easily separated from the central portion. On the other hand, the iron which occluded hydrogen presented a surface of a light-brownish colour at first, which gradually became dark and finally almost black as the electrolysis continued longer, but always became bright when wiped with soft cloth. This colour disappeared by heating, and the wire recovered its ordinary iron tint.

In the first method tried, I fixed the wire in the axis of an iron tube. One end of the tube was clamped to the wire and heated in a charcoal furnace, and five junctions were made in different parts of the wire, as described in a previous paper by Prof. Knott and myself.* But after weeks of experimenting, the difficulty of determining the places at which the important change takes place compelled me to abandon this method. Next I made five separate triple junctions, each of thin iron wire, thin German-silver wire, and the iron wire charged with one of the gases, and put them in an oil-bath, and observed the five different thermo-electric currents in turn. But with this arrangement it was difficult to follow the change closely.

* *Journ. of the Coll. of Sc. Imp. Univ. Tokio*, vol. iv. pt. ii.

Finally, I took only one such junction, of which the thin iron and thin German-silver formed a thermometric junction, while the thick charged wire and the thin iron formed a thermo-electric junction. In this manner I tried the above three methods, one after the other. The CO-iron line (Pl. I. *a*) was obtained by the second method and the $CO_2$-iron line (Pl. I. *b*) by the third. On comparing the results obtained by the second and the third methods with those obtained by the first, I found them to be fairly concordant.

The galvanometer employed was a high-resistance double-coiled one. It was gauged before each new experiment. In taking the readings I observed first, the direct and reverse currents of the thermometric circuit, then the direct and reverse currents of the thermo-electric circuit, and then again the direct and reverse currents of the thermometric circuit. These three pairs of readings determined by themselves one point in the curve showing the relation between the thermo-electric and thermometric deflections. Two gas burners of different sizes were employed to make the rise of temperature as uniform as possible. This was 9 degrees during the three pairs of readings, or five divisions in the scale of deflections. The thermo-electric current passed through a resistance of 1000 ohms, while the thermometric passed through 2000 ohms and a shunt of 150 ohms, the resistance of the galvanometer being 1560 ohms. Curves of cooling were also obtained in each case. During the slow cooling I made the necessary calculations and drew the curve, so that I did not waste time continuing the experiment after the intersection of the heating and cooling curves. The CO-iron and $CO_2$-iron were cut from different samples of equal thickness.

Apart from the main object of the research, I made many other experiments. For instance, I compared the thermo-electric currents of two pieces cut from the same iron wire, annealed with different degrees of care, and found that the thermo-electric line of the more carefully annealed wire lies parallel with, and a little below, the line of the less carefully annealed wire. The line of a hard drawn wire lies, at low temperatures, above that of the annealed wire, and comes in contact with it at a temperature of about 250°. This shows that the thermo-electric current runs through the hotter junction from the less carefully annealed iron to the more carefully annealed, and from the hard drawn to the annealed. The wire, which was simply made yellow-hot in air by a dynamo current, was found to have its line nearly parallel to that of the annealed, and presented no discontinuity as in the case of the wire which had occluded the gas. The effect of the dynamo current was smaller than that of less careful annealing.

In the experiments with iron wires occluding these various gases, the peculiarity was sometimes remarkably well defined, but sometimes imperfectly so at high temperatures. In the latter cases the heating and cooling curves also intersected at high temperatures. The change of colour of the surface of hydrogenised iron by heating had suggested to me the importance of the condition of the *surface* of the affected wires. In the earlier experiments, the ends of the wires where the junctions were made were rubbed with sandpaper to make the contact surer. Later, I left them untouched, and the peculiarity was always well defined and took place at low temperatures. Thus it seems clear that the effect of occlusion is largely confined to the surface.

Invariably the cooling curve of the charged iron lay above the heating curve, while in annealed iron and iron in which gas was not occluded, the curve of cooling was the lower.

In the case of CO-iron (*a*, Pl. I.), the electro-motive force between thin iron and CO-iron rose steadily with the temperature to about 180°, and then suddenly decreased as the temperature rose a few degrees; but as the heating continued it began again to rise steadily, giving a curve entirely different from the earlier one. Thus it is evident that carbonic oxide suddenly escapes at about 180°, the wire regaining a stable condition at about 190°. During cooling, the curve runs without break between these two temperatures. The cooling curve lies above the second part of the heating curve, but below the first part. Possibly the end of the wire partially recovers some portion of occluded gas from neighbouring cooler parts of the wire. Another annealed wire, cut from the same piece, but not made to occlude gas, gave a heating curve running parallel to the former curve, but passing between its first and second parts, and on cooling gave a curve parallel and close to its own heating curve. From this indirect comparison, it may be concluded that before the sudden escape of the gas the thermo-electric current runs through the hotter junction from the charged iron towards the normal iron, but after the escape reverses its direction. I regret that I did not verify this by direct experiment, *i.e.*, without the use of the third thin wire.

The $CO_2$-iron (*b*, Pl. I.) behaved like ordinary iron, but between 140° and 150° the gas seemed suddenly to escape. From 150° to 250° it reached a second time a stable condition. The cooling curve also lay above the heating curve, and cut the latter at the temperature where the gas escaped.

The accompanying curves (Pl. I.) show the relation between the temperature of the hotter junction in centigrade and the electromotive force between thin iron and the charged iron wires on an arbitrary scale of deflections. The temperature of the hotter junctions were first measured by the thermometric current, and then reduced to the centigrade scale. The galvanometer was gauged at the beginning of each experiment. In the experiment with the CO-iron, the cold junction was kept at 15°·8; and in the case of the $CO_2$-iron, the cold junction was kept at 20°·5.

To sum up, iron saturated with carbonic oxide or carbonic acid gas behaves thermo-electrically as a metal until a certain temperature is reached. At this temperature the gas begins to escape till a certain other temperature is reached; and from this up to higher temperature the metal is in a new stable condition. During cooling, however, the iron recovers a part of the gas, and remains stable throughout. The cooling curve intersects the heating curve at about the mean temperature corresponding to the escape of gas.

In CO-iron, gas escapes at between 180° and 190° C. The thermo-electric current flows through the hotter junction from CO-iron to the normal iron at a temperature lower than the critical temperature just named, but above this temperature it flows in an opposite direction. After partial recovery of the gas on cooling, the current is very feeble.

In $CO_2$-iron, the gas escapes at between 140° and 150° C. Here there is a suggestion that the wire on cooling recovers the gas more than it can sustain in that high temperature, so that the curve shows a swelling up at the beginning of cooling. I observed the same phenomenon in many other experiments on hydrogenised iron. It must be here noticed that the amount of carbonic acid occluded in iron is very much less than that of carbonic oxide or hydrogen.

## II. *Thermo-Electric Properties of Hydrogenised Iron.*

For the direct measurement of the thermo-electric current, I divided into two a piece of iron wire of diameter $\frac{1}{5}$ mm. One of the pieces was put for three hours into the electrolytic bath, through which a current from three Daniells was passed. The thermo-electric current was measured, as before, by a double-coiled delicate galvanometer, which was gauged immediately before each experiment. The temperature of the hotter junction was measured by a thermometer dipped into a test-tube filled with oil, which again dipped into a large oil-bath with a metal cover—the junction being dipped into the test-tube along with the thermometer. The bath was heated from below by gas, the rise of temperature being controlled by two burners. Before the reading of the scale of the galvanometer the burner was withdrawn, and the oil in the bath briskly stirred. After the thermometer had risen a few degrees, the thermo-electric circuit was closed, and after a few seconds the thermometer was read. Then the right and left deflections on the galvanometer were read, and the thermometer read again. These readings formed one set of observations. The maximum rise of temperature during one such set was 2° and the minimum rise 0°·8. This process was found more convenient and accurate than the process involving the use of a thermometric junction.

The thermo-electric circuit with the galvanometer contained no extra resistance or shunt. The galvanometer coils were joined in multiple arc, which reduced the galvanometer resistance to one-fourth. Yet the deflections were very small, ranging from four divisions of the scale to fourteen divisions, which correspond respectively to 8 and 28 microvolts. This smallness of the thermo-electric current and of the galvanometer deflections rendered the results of the experiments a little irregular. The following experiment is one of two, which were made after many preliminary experiments of the same kind. The temperature of colder junction was 26° C.:—

| Temperature of Hotter Junction. | Galvanometer Deflection. | Deflection Reduced to Microvolts. | Temperature of Hotter Junction. | Galvanometer Deflection. | Deflection Reduced to Microvolts. |
|---|---|---|---|---|---|
| | Heating. | | | | |
| 26·00 | 4·00 | 8·28 | 201·00 | 9·50 | 19·50 |
| 30·00 | 4·10 | 8·48 | 210·75 | 9·60 | 19·71 |
| 44·75 | 5·00 | 10·28 | 220·75 | 10·75 | 22·10 |
| 67·75 | 5·25 | 10·88 | 230·50 | 11·25 | 23·10 |
| 79·75 | 5·80 | 11·99 | 241·00 | 13·25 | 27·16 |
| 99·00 | 6·75 | 13·87 | 250·40 | 14·00 | 28·77 |
| 112·25 | 7·00 | 14·33 | | | |
| 124·75 | 7·00 | 14·33 | | | |
| 128·00 | 7·00 | 14·33 | | | |
| 133·00 | 6·85 | 14·06 | | | |
| | | | | Cooling. | |
| 138·00 | 6·95 | 14·13 | 251·50 | 12·75 | 26·12 |
| 143·75 | 7·20 | 14·75 | 249·20 | 12·75 | 26·12 |
| 148·75 | 7·10 | 14·55 | 245·50 | 12·15 | 24·90 |
| 153·50 | 7·20 | 14·75 | 241·00 | 12·15 | 24·90 |
| 157·25 | 7·25 | 14·88 | 231·50 | 11·40 | 23·40 |
| 163·50 | 7·75 | 15·90 | 218·50 | 11·00 | 22·60 |
| 168·50 | 7·85 | 16·10 | 189·50 | 10·00 | 20·55 |
| 173·50 | 8·30 | 17·05 | 162·00 | 8·75 | 19·93 |
| 181·75 | 8·60 | 17·67 | 120·50 | 8·25 | 16·93 |
| 191·50 | 9·00 | 18·47 | 83·50 | 7·85 | 16·10 |

The direction of the thermo-electric current is from normal iron to that charged with hydrogen, through hotter junction. Looking at the curve (Pl. II.), the march of the thermo-electric current is almost a straight line from the temperature of the colder junction up to 95°. From 95° to 140° the curve becomes almost horizontal, indicating an escape of hydrogen at this stage of the heating. From 140° up to 250° the curve is regularly convex towards the temperature ordinate. During cooling the curve has at first a greater and then a less inclination to the temperature ordinate than the curve of heating, and cuts the latter at about 230°. Thus the thermo-electric current between the heating and cooling wires changes its direction at 230°. The curve has a strong resemblance to those of iron charged with carbonic oxide or acid; but the difference lies in this, that the curves during cooling and heating do not intersect at the temperature of rapid escape of gas, but at a temperature much higher than this. The direction of the current was the same as that of the indirect preliminary experiments.

### III. *Resistance of Hydrogenised Iron.*

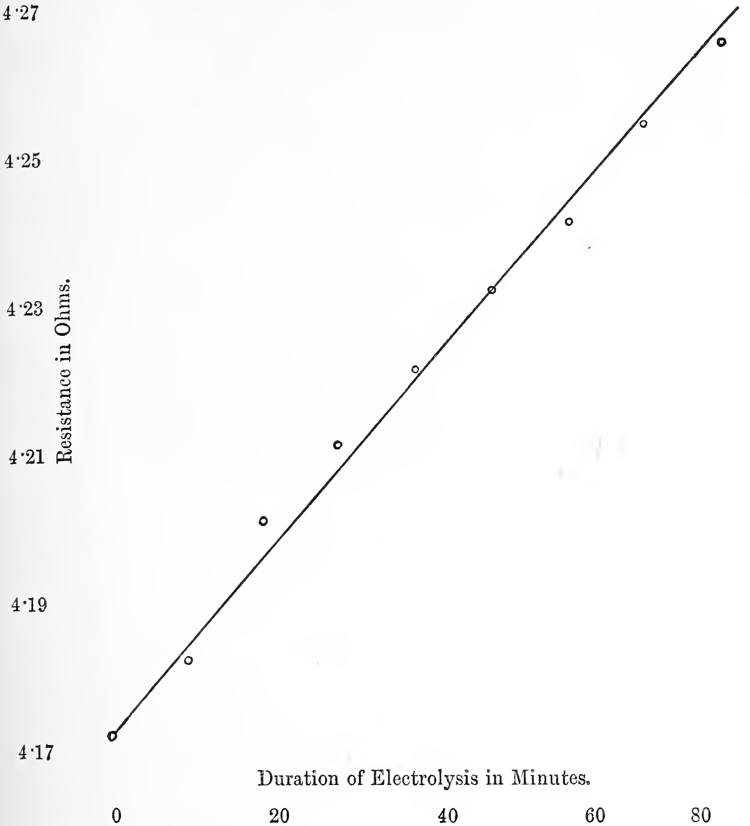
In this part of the experiment I measured the change of resistance due to the hydrogen charged. The charge, when too small to be measured by weight, was roughly measured by the time of electrolysis, the strength of the current being kept practically constant. When iron is put into an electrolytic bath, the hydrogen liberated upon its surface is gradually absorbed by it, though some portion, of course, escapes through the water. Thus the duration of electrolysis can give only a very rough measurement of the charge.

When the duration of electrolysis is short, say twenty or thirty minutes, the surface of the wire remains bright when it is allowed to dry; but when the duration is much longer, say a day or two, the surface becomes covered with black dust which adheres to the cloth when it is wiped, while the wire is still wet. The amount of this dust, though small, affects the weight of the wire to the extent of some milligrammes.

In the experiment, a hard-drawn iron wire $\frac{1}{5}$ mm. in diameter and 1 metre in length was taken, and, after being rubbed with emery-paper, was put into the electrolytic bath. The current was supplied by three Daniells, and was measured on a tangent galvanometer. The wire was taken out every ten minutes, and was carefully wiped with a soft cloth. Its resistance was then measured by an ordinary resistance box and a delicate galvanometer. The following are the results of the measurement, the temperature being that of the room, 25° C.:—

| After | Resist. in Ohms. | After | Resist. in Ohms. |
|---|---|---|---|
| 0 min. | 4·176 | 50 min. | 4·235 |
| 10 ,, | 4·186 | 60 ,, | 4·244 |
| 20 ,, | 4·204 | 70 ,, | 4·257 |
| 30 ,, | 4·214 | 80 ,, | 4·268 |
| 40 ,, | 4·224 | | |

These figures show (see also the curve) that the resistance increases steadily with the time of electrolysis. Hence, assuming the charge of hydrogen to be proportional to the duration of electrolysis, we may regard the change of resistance for such small charges to be proportional to the charge. But with a greater charge, the resistance seems to diminish as the charge is increased.




Another piece of hard-drawn iron wire, of the same diameter and same length as before, was put into the bath for one whole day. When the wire was taken out, it was carefully dried and wiped. In this case, the original weight of the wire was 0·2170 grammes and the weight after the occlusion was 0·2192 grammes, so that there was an increase of weight to the amount of 2·2 milligrammes. This being assumed to be due to the hydrogen occluded, corresponds roughly to a volume of gas (at atmospheric pressure) of 25 cubic centimetres, or 790 times that of the wire. The original resistance was 4437 ohms at 24°·7. After the occlusion the resistance was

4418 ohms at 25°. Thus there was a decrease of resistance to the amount of 0·019 ohms, or ·43 per cent.

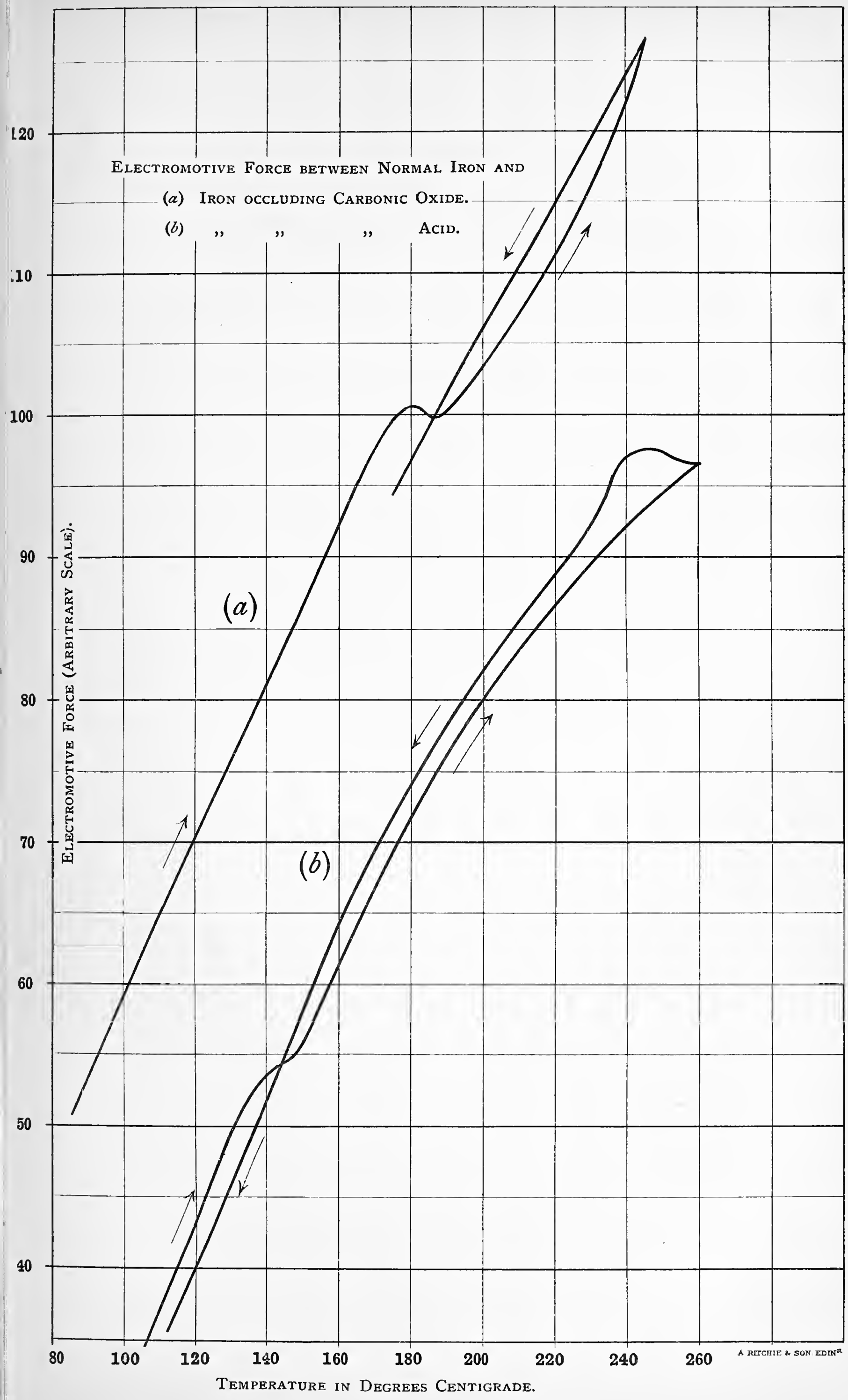
Another wire, of diameter $\frac{1}{2}$ mm. and of the same length as before, was put into the bath for the same length of time. The original weight was 1·5460 grammes, and the final weight 1·5494 grammes; so that, under the same assumptions, the volume of the charge of hydrogen was 38 cubic centimetres, or about 200 times that of the wire. The resistance decreased from 0·732 ohms to 0·728 ohms, *i.e.*, by 0·55 per cent.

Since a wire of diameter $\frac{1}{2}$ mm. weighs 1·546 grammes, a wire of equal length of diameter $\frac{1}{5}$ mm. will weigh ·247 grammes, provided they are of the same material. But the one experimented with weighed only ·217 grammes, and increased by 2·2 milligrammes. Hence the thicker one would have increased its weight under the same treatment by 2·5 milligrammes. In the experiment the thicker wire increased its weight under an approximately similar treatment by 3·4 milligrammes. Hence the thicker wire absorbed more hydrogen than the thinner wire in the ratio of 34 to 25, or 1·36 to 1. Simultaneously, the resistance of the thicker wire diminished more than did that of the thinner wire in the ratio of 55 to 43, or 1·3 to 1. Considering the minuteness of the quantities measured, and remembering that the thin and thick wires were probably not of the same material, we may regard these results as being in satisfactory agreement.

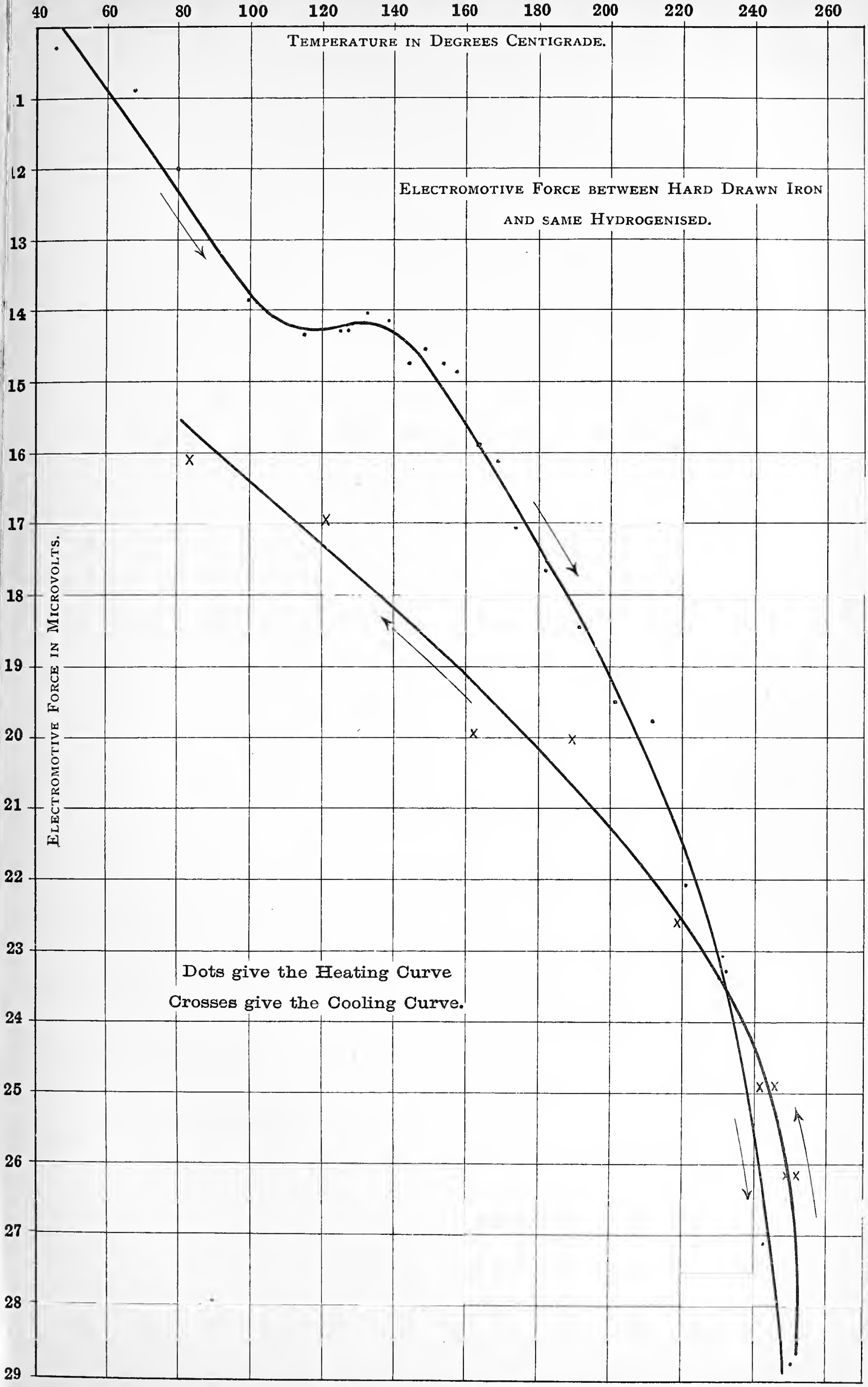
The conclusion indicated so far seems to be that, at first, for small charges of hydrogen, the resistance of the wire increases proportionately to the charge; but that, when the charge is great, the resistance is diminished.

The experiments which form the subject of this paper were all made in the Physical Laboratory of the Imperial University, Tokyo, Japan.

MR. KIMURA ON ELECTRICAL PROPERTIES OF IRON.—PLATE I.



MR. KIMURA ON ELECTRICAL PROPERTIES OF IRON.—PLATE II.




A. RITCHIE & SON EDIN[R]

# Note on the Antecedents of Clerk-Maxwell's Electro-dynamical-Wave-Equations. By Prof. Tait.

(Read April 2, 1894.)

The first obvious difficulty which presents itself, in trying to derive Clerk-Maxwell's equations from those of the elastic-solid theory, appears in the fact that the latter, being linear, do not impose any relations among simultaneous disturbances. Thus, for instance, they indicate no reason for the associated disturbances which, in Maxwell's theory, constitute a ray of polarised light. Hence it appears that we must look on the vectors of electric and magnetic force, if they are to be accounted for on ordinary dynamical principles, as being necessary concomitants, qualities, or characteristics of one and the same vector-disturbance of the ether, and not themselves primarily disturbances. From this point of view the disturbance, in itself, does not correspond to light, and may perhaps not affect any of our senses. And the very form of the elastic equation at once suggests any number of sets of two concomitants of the desired nature, which are found to be related to one another in the way required by Maxwell's equations.

For the moment, as sufficiently illustrating the essential point of the above remarks, I confine myself to disturbances, in the free ether, such as do not involve change of volume. The elastic equation is

$$\ddot{\theta} = -a^2 \nabla^2 \theta$$

with the limiting condition

$$S \nabla \theta = 0 \ . *$$

[Had not this condition been imposed, the dynamical equation would have involved, on the right, the additional term

$$(a^2 - b^2) \nabla S \nabla \theta .]$$

* Stokes "On the Dynamical Theory of Diffraction," *Camb. Phil. Trans.*, ix. (1849).

From any vector satisfying these equations let us derive (by means of the operators $d/dt$ and $a\nabla$, which are the only ones occurring in the equation of motion) the concomitants

$$\epsilon=\dot{\theta}, \quad \mu=-a\nabla\theta;$$
$$\text{or} \quad \epsilon=\ddot{\theta}, \quad \mu=-a\nabla\dot{\theta}, \quad \&c., \&c.$$

and we have between them Clerk-Maxwell's equations

$$\dot{\epsilon}=a\nabla\mu, \quad \dot{\mu}=-a\nabla\epsilon,$$

with the conditions

$$S\nabla\epsilon=0, \quad S\nabla\mu=0.$$

The extension to dielectrics, whether they be isotropic or not, is obtained at once:—and it secures (in the latter case) all the simplicity which Hamilton's linear and vector function affords. Thus the properties of double refraction, wave-surfaces, &c., follow almost intuitively.

When we come to conducting bodies, we have to introduce further conditions. But I do not enter on these at present, as the problem is essentially altered in character. Nor do I, for the moment, discuss the bearing of the above notions upon the profound question of the possible *nature* of electricity and of magnetism.

There is a sort of analogy to the above, in the case of sound. For it is not the (vector) disturbance of the air which affects the sense of hearing, but the (scalar) concomitant change, or rate of change, of density.

Thus, possibly, the widely different results obtained by observers of the alteration of plane of polarisation in diffracted light, may *all* really be in accordance with Stokes' splendid investigation:—if we look upon light as an effect produced by the concomitants of the ether disturbance, and not directly by the ether disturbance itself.

## The Rise and Progress of Anthropology. By Robert Munro, M.A., M.D. (With a Plate.)

(An Address delivered at the request of the Council, May 7, 1894.)

However far back written records conduct us in an investigation of the early history of mankind, it can no longer be maintained that they cover but a small portion of man's existence on the globe. That human racial characters were broadly marked, some 6000 years ago, has been surmised from an analysis of the ancient wall paintings of Egypt. Thus, in the tombs of the kings at Thebes are to be seen, to this day, coloured and highly expressive portraits of the four principal races who then frequented the Nile valley; and it is a remarkable fact that their distinguishing peculiarities, as depicted in the conventional eye and reddish-brown colour of the Egyptian, the fair-skinned and blue-eyed Lybian, the aquiline profile of the Semite, and the thick lip and curly hair of the Negro, are equally descriptive of their modern representatives. But if, during this long period, physical changes in these races have been so slight as to be almost inappreciable at the present time, what, it may be asked, must have been the duration of mankind in prehistoric times, whilst these persistent distinctions were being worked out under the influence of natural laws? From this point of view commemorative inscriptions, pictorial paintings, hieroglyphs, traditions, &c., lead us scarcely beyond the threshold of the dim vista which is made to converge in the remote past at a time when the ancestors of the white-, black-, and red-skinned people were one undivided stock. Similar deductions have been drawn from a study of the elements of speech, growth of culture, religious customs, and other deep-seated phenomena of human civilisation. Hence it has been argued that these general considerations alone furnish *prima facie* grounds for believing that, long before the rise of the earliest empires of antiquity, human characteristics had already been differentiated.

It was not, however, till a time to which the recollection of many now living extends that the scattered elements of Anthropology assumed sufficient coherency to be formulated into a science, and to give expression to a theory of man's origin more in accordance with observed facts than that which regarded him as the sudden product of a creative fiat. As time progresses, the scientific discoveries which gave rise to this important change of opinion are apt to be forgotten, in the midst of the engrossing social and intellectual problems which are daily springing up in the ever-widening field of human activity. Let me, therefore, very briefly bring to your recollection some of the more outstanding features of these discoveries, and of the intellectual upheaval which so speedily led to the recognition of Anthropology as an important branch of human knowledge.

Prior to the publication of Sir Charles Lyell's work on *The Geological Evidences of the Antiquity of Man*, isolated discoveries were recorded, from time to time, in different parts of Europe, disclosing facts which, in the opinion of a few savants, could only be accounted for by assigning to Man a higher antiquity than was then the current opinion. These discoveries consisted, for the most part, of the remains of Man—bones and industrial relics—associated with the bones of extinct animals, in undisturbed deposits of Quaternary times. The reception at first given to this class of evidence in scientific circles may be estimated from the following notes on a few of the earlier discoveries. It was about the beginning of the second quarter of this century that Kent's Cavern, near Torquay, first became a subject of interest, owing to the researches of the Rev. J. MacEnery, who asserted that he found in it flint implements, associated with the bones and teeth of extinct animals, below a thick continuous sheet of stalagmite. But the legitimate inference from these facts, viz., that Man was contemporary with these animals, and lived before the deposition of the stalagmite, had little chance of being accepted when opposed by the teaching and authority of so famous a geologist as Dr Buckland, author of the *Reliquiæ Diluvianæ* and *The Bridgewater Treatise on Geology and Mineralogy.*

The facts on which Mr MacEnery based his conclusions were verified by fresh excavations in the cavern made by Mr Godwin-

Austen, F.G.S., in 1840, and by a committee appointed by the Torquay Natural History Society in 1846. Papers embodying the results of these investigations were read at the Geological Society of London, and at the meeting of the British Association for 1847. But, according to Mr Pengelly, the reception accorded to these researches was not encouraging, and the inconvenient conclusions arrived at "were given to an apathetic, unbelieving world."

Another discovery of a similar character was the Windmill-Hill Cavern, at Brixham, explored in 1858, under the auspices of a committee appointed by the Royal and Geological Societies of London. The first paper on the result of this investigation was read by Mr Pengelly in September 1858, at the meeting of the British Association then held at Leeds, in which it was announced that "eight flint tools had already been found in various parts of the cavern, all of them inosculating with bones of mammalia at depths varying from 9 to 42 inches in the cave-earth, on which lay a sheet of stalagmite from 3 to 8 inches thick, and having *within* it and *on* it relics of the lion, hyena, bear, mammoth, rhinoceros, and reindeer."

This paper, to use the phraseology of Mr Pengelly, produced a decided "awakening," besides indirect results of the highest importance.

Nor did analogous discoveries on the Continent fare much better. In 1829 Dr Schmerling commenced his memorable researches in the caverns of the province of Liège. The evidence of man's antiquity revealed by his investigations consisted of flint implements, and the actual remains of human skeletons, among which was the famous Engis skull, associated with the bones of hyena, lion, rhinoceros, mammoth, reindeer, and cave-bear. This indefatigable explorer published an account of his discoveries in two splendid volumes, with an atlas of 74 plates (1833–4), in which, in the most unequivocal language, he contended for the contemporaneity of Man with these extinct animals; but, owing chiefly to the influence of the doctrine taught by the great naturalist Cuvier, his opinions and arguments did not receive the attention they merited. Remains of Man found in caverns were thus summarily disposed of by Cuvier:—"On a fait grand bruit, il y a quelques mois, de certain fragments humains trouvés dans des cavernes à ossements de nos provinces méridionales, mais il suffit qu'ils aient été trouvés dans les cavernes pour qu'ils rentrent dans la règle" (*Discours sur les revolutions du Globe*, p. 89).

The discovery by M. Boucher de Perthes of rude flint implements, associated with the bones of the mammoth and other extinct animals, in the ancient gravel beds of the valley of the Somme, at various levels considerably above the present highest flood-marks of the river, equally failed to attract scientific attention. An account of his researches, under the title *Antiquités Celtiques et Antédiluviennes*, was published in 1847, but for upwards of ten years it lay absolutely unheeded.

Interest in these novel speculations became now greatly enhanced in consequence of equally important and far-reaching discoveries in the collateral sciences. The entire borderland of geology and anthropology was being better understood, especially as regards the remarkable glacial phenomena of Quaternary times in northern and western Europe; and archæology proper, independently of its new-born palæolithic phase, had acquired a wider significance, owing to the more rigid and scientific methods adopted in its study. The Scandinavian savants, despairing of being ever able to elucidate the early history of their country by means of the ancient Sagas and other traditionary sources, were now subjecting the archaic remains, so profusely scattered over the country, to the most crucial tests which scientific ingenuity could devise. All departments of knowledge—geology, hydrography, conchology, zoology, botany, and ethnology—were enlisted in this national work. In this manner, and with such resources, they examined peat mosses, graves, megalithic monuments, refuse heaps, and, in short, everything that was likely to throw light on the culture and civilisation of the prehistoric people of their country. The successive discoveries of Køkkenmøddings in Denmark and lake-dwellings in Switzerland, with the vast and varied wealth of prehistoric materials which they disclosed, now also began to attract universal attention.

While these problems and numerous side issues were being discussed, the scientific world was startled in 1859 by the publication of Charles Darwin's *Origin of Species.* In this work the author advocated, with singular completeness and ability, that the various species of plants and animals now extant, and being continued by the ordinary laws of generation, had been derived from pre-existing forms by secondary causes—a process which he designated under the name of *Natural Selection.* The key to this theory is to be found in the severe struggle for existence which all organisms have to maintain, not only against their natural enemies, but against the overcrowding of their own species. The outcome of a contest under conditions where it is only possible for a limited number to find the means of subsistence, is the survival of the fittest and the extinction of the weakest. In this manner Mr Darwin traced the origin of Man through a series of intermediate forms back to protoplasm, without the intervention of repeated cataclysms and special creative dramas, as was generally held by the earlier geologists. "As all the living forms of life," says Mr Darwin, "are the lineal descendants of those which lived long before the Cambrian epoch, we may feel certain that the ordinary succession of generation has never once been broken, and that no cataclysm has desolated the whole world" (*Origin of Species*, p. 428).

Like all great discoveries, the grandeur of Mr Darwin's conception lay in the simplicity and transparency of its truth; and as a small particle leavens the mass, so the words "struggle for existence" and "survival of the fittest" set the whole philosophical world into a ferment. Indeed, it is impossible to exaggerate the profound effect produced on his fellow-men by the doctrine thus taught by Mr Darwin. Many of the greatest naturalists of the day at once discarded their former creeds and adopted the evolution theory of life; and at the present time it may be well asked who and where are its opponents? Evolution was then by no means a new idea, but hitherto no naturalist had formulated a *modus operandi* of its laws. Lamarck believed in the development of the higher animals, but he adduced no evidence in support of his belief beyond the vaguest hypothesis. On the other hand, Cuvier, who had the amplest evidence daily before his eyes, was so blinded by his preconceived notions, that he failed to take advantage of the strange palæontological materials among which he worked.

Foremost among the galaxy of eminent men who took part in the exciting controversies which the "Origin of Species" gave rise to was the celebrated geologist, Sir Charles Lyell, whose work on the

Antiquity of Man greatly helped to consolidate the doctrines of Anthropology. In this work the author collected the previously recorded materials bearing on the early history of Man from all parts of the world. The effect of its accumulated details was so overwhelming that there could no longer be any doubt that the existence of humanity on the globe must be relegated far back into the Quaternary period. With the general acceptance of the doctrine of evolution and man's great antiquity terminates what may be called the struggling period of Anthropology.

Henceforth a new impetus was given to the study of this science by the conviction that in the meanest traces of man's early career were to be found more important materials for a history of humanity and civilisation than in all the treasures that could be collected from the ruins of the greatest empires of the historic world. The wide morphological gap between Man and the other animals still living suggested a correspondingly long period for his development, in the course of which it was expected that some evidence of the stages through which he had passed might have become stereotyped in the geological records. Where to find and how to interpret these records were now the chief problems at issue; and to their solution the savants of all countries braced themselves with an energy that augured final success. Societies were founded in London, Paris, and other centres of intellectuality, for the express purpose of following up the new-found trail of humanity; and to popularise and disseminate their doctrines, numerous periodicals and special works were published. In the year 1865, at a special meeting of the Italian Society of Natural Science held at Spezzia, was founded the "Congrès International d'Anthropologie et d'Archéologie Préhistoriques," the first meeting of which was held in the following year at Neuchâtel. Subsequent meetings have been held at Paris (1867), London (Norwich, 1868), Copenhagen (1869), Bologna (1871), Brussels (1872), Stockholm (1874), Buda-Pesth (1876), Lisbon (1880), Paris (1889), and Moscow (1892). The published proceedings of these congresses contain the most complete records of the progress of the science, especially as regards Europe. After the cloud of scepticism which enveloped its early and evolutionary stages had been swept aside, Anthropology found a footing at the British Association, at first as a sectional department, but since

1884 it became expedient to devote a special section for the exclusive consideration of its doctrines.

As already remarked, it was the coalescence of the greatly extended power of deciphering unwritten records with the almost coincident teaching of Darwin which first enabled the antiquary to look beyond the horizon of historic vision, and so to discover materials for a science of Anthropology. So long as it was maintained that Man had been ushered on the arena of life specially equipped, morphologically and teleologically, for the struggle of existence, there was no room for such a science, as its range would be necessarily restricted to the operations and modifications of mankind during the last five or six thousand years—a field already sufficiently covered by the ordinary historical methods of research. From the new standpoint, Anthropology has a much wider scope, and embraces the origin, development, and civilisation of mankind. Its object is to trace the career of Man through space and time, amidst the vicissitudes of his ever-changing environments, during the ages which have elapsed since he first diverged from his quadrupedal congeners. During this long period there were many influences at work, all of which have to be carefully noted; the causes which led to the physical and mental endowments which gradually transformed him from *Animal brutum* to *Homo sapiens;* the methods and processes by which he discovered and utilised the forces of nature, and constructed a system of civilisation on the principles of intelligence; and finally, the means by which he learned to distinguish between good and evil, in consequence of which he became a responsible being, and laid the foundations of a science of conscience and ethics.

To analyse and systematise the evidences on which these momentous issues are based is the special province of Anthropology. Whatever opinion may be formed as to the adequacy of the argumentative materials already collected in support of the conclusions arrived at, one thing is certain, that they cannot be ignored. They are culled from the widest possible range of mental and physical phenomena, and are rapidly accumulating. On the present occasion it will be sufficient for my purpose to take a bird's-eye view of them under the following heads:—(1) Ethnology, (2) Language, (3) Structural relationship of Man with other living Organisms, (4) Fossil Man, (5) Handicraft Products of Man, (6) and lastly, The bearing of Geology on the Prehistoric Remains of Man.

I do not propose to discuss here the amount and respective values of the materials so classified, but merely to give a few illustrative examples of the manner in which they are brought together through these different channels, and made to fit complementary niches in the construction of the science of Anthropology.

(1) *Ethnology.*—In regard to ethnology, it is almost unnecessary to say anything. The geographical distribution of the various races of Man, the physical peculiarities of the bodies and features,—the conformation of the skull, the size and structure of the brain, the colour of the skin, eyes, and hair,—together with the products of different civilisations scattered over the globe, are amongst the most essential elements which enter into this science. At the present time, indeed, great prominence is given to the collection and assortment of such ethnological materials brought by travellers from all parts of the world.

(2) *Language.*—Knowledge may be communicated from one individual to another by gestures, sounds, pictures, and characters or letters representing definite ideas, according to a pre-arranged system; and it belongs to the science of Anthropology to trace the growth of all these methods to their primary sources. The value of language when stereotyped in books and inscribed stones, such as the hieroglyphic and pictorial monuments of Egypt and the cuneiform tablets of Assyria and Babylonia, is so apparent that I need not dwell on this phase of the subject. On the other hand, spoken language is too transient to be reckoned of much consequence in determining the racial distinctions of mankind. The geographical distribution of a language does not always coincide with that of the people who invented it; and, indeed, a given speech may altogether cease to be a living means of intercommunication, while its original inventors survive and continue to flourish under one borrowed from a different race. The fact that the Celtic language, which formerly prevailed over a large area in Western Europe, is now only to be found in one or two isolated corners, lends no support to the theory that a similar fate has overtaken the people who first introduced it. If we look underneath the superficial crust of modern civilisation, even in the most Saxonised part of England, we find the change of

speech to be in many instances merely a gloss over the more persistent racial characters of a former people. *Cœlum non animum mutant, qui trans mare currunt.* It is indeed seldom that the most evanescent peculiarities of a people disappear altogether without leaving some traces behind them: even the fragments of a vanished language, when carefully looked for, will be found fossilised in the names of the outstanding features of the country—its mountains, valleys, rivers, lochs, forests, &c. But it is the results obtained through recent methods of palæo-linguistic research that more especially interest us as anthropologists. These results may be better illustrated by a well-known example. A glance at the structural elements of Italian, French, and Spanish is sufficient to show that these languages are direct descendants of Latin; and had this language been absolutely lost, modern philologists could, to a large extent, have reconstructed it. By the application of their analytic methods to the inscribed materials dug out of the ruins of protohistoric monuments, philologists have been able to extend this field of research far back into prehistoric times. They have most conclusively shown that the so-called Indo-European languages, comprising Old Celtic, Latin, Greek, Gothic, Old Russian, Old Persian, Sanskrit, &c., have descended from one common language. The existence of this language, however far back it may be removed from its varied offspring, necessarily implies a people who spoke it; but who these primitive Aryans were, where they lived, and whence they came, are amongst the most controverted problems of the present day. Similarly, these linguistic archæologists are now successfully pushing their investigations into the Accadian or pre-Semitic period, which underlies the civilisations of Assyria and Babylon.

(3) *Structure of Man.*—The striking correspondence between the bodily structure of Man and that of some of the higher animals, such as the anthropoid apes, could hardly have escaped the attention of reflective man in all ages; and I have no doubt that long before Huxley published his work on *Man's Place in Nature*, vague ideas of this kind had flitted across the brain of many a bygone philosopher. But all these premonitory glimmerings of the truth would be probably smothered, as it were in embryo, by the overpowering influence of prejudices founded on other issues. It is

thus referred to by Darwin:—"It is notorious that Man is constructed on the same general type or model as other animals. All the bones in his skeleton can be compared with corresponding bones in a monkey, bat, or seal; so it is with his muscles, nerves, blood-vessels, and internal viscera. The brain, the most important of all the organs, follows the same law, as shown by Huxley and other anatomists. Bischoff, who is a hostile witness, admits that every chief fissure and fold in the brain of Man has its analogy in that of the orang; but he adds that at no period of development do their brains perfectly agree; nor could perfect agreement be expected, for otherwise their mental powers would have been the same" (*Descent of Man*, p. 6). This correspondence becomes still more apparent when we examine the phenomena of the fœtal life of animals. Not only does the human embryo start from an ovule similar to, and indistinguishable from, that of other mammals, but its subsequent changes follow on precisely the same lines. Moreover, all the homologous organs in the full-grown animals, as the wing of a bird, the flipper of a seal, and the hand of Man, are developed from the same fundamental forms. "Without question," says Professor Huxley, "the mode of origin and the early stages of the development of Man are identical with those of the animals immediately below him in the scale" (*Collected Essays*, vol. vii. p. 89).

The illustrious von Baer, who first directed special attention to embryology, formulated a law to the effect that the structural differentiation in fœtal development was from a general to a special type. Haeckel, looking at the same phenomena from a different standpoint, came to the conclusion that the development of the individual is a recapitulation of the historic evolution of the race. This is a most astounding statement; and, if true, the study of embryology should supply the anthropologist with a much shorter way to the goal of his inquiry—a way by which the progressive phases of man's corporeal structure would be reduced to the compass of an experimental illustration within the precincts of the laboratory. Not being a practical physiologist, I am unable to determine the precise value to be assigned to this analogy between the two evolutions, but, on other grounds, I should say that it is true only in a very general way. If embryology is as conservative of energy as other organic processes, I would expect that some minor links would have dropped out altogether in passing to higher results. Nature's operations are full of short cuts. As a parallel case, let me cite the instinct which makes a bee fix on a hexagonal cell, or which leads a bird to migrate in winter, both of which must be regarded as originally acquired through the ordinary means of natural selection, but which ultimately have become transmitted directly through heredity, altogether independent of their earlier evolutionary stages.

Another fertile source of arguments in support of the theory of man's descent from the lower animals is to be found in the rudimentary organs described by anatomists as normally present, or occasionally to be met with, in Man. Such organs as canine teeth, the coccyx, the inter- and supra-condyloid foramina of the humerus, the appendix vermiformis, remnants of some muscles, &c., &c., are apparently useless in the human economy, but their homologues in other animals have special functions assigned to them. But, indeed, the homological structure of the entire human body is utterly inexplicable on any other hypothesis.

"Thus we can understand," to quote Darwin's words once more, "how it has come to pass that man and all other vertebrate animals have been constructed on the same general model, why they pass through the same early stages of development, and why they retain certain rudiments in common. Consequently we ought frankly to admit their community of descent; to take any other view is to admit that our own structure, and that of all the animals around us, is a mere snare laid to entrap our judgment. . . . . It is only our natural prejudice, and that arrogance which made our forefathers declare that they were descended from demigods, which leads us to demur to this conclusion. But the time will before long come when it will be thought wonderful that naturalists, who were well acquainted with the comparative structure and development of man and other animals, should have believed that each was the work of a separate act of creation."—(*Descent of Man*, p. 25.)

(4) *Fossil Man.*—The difficulty of assigning a definite age to the osseous remains of ancient Man which have hitherto come to light, owing partly to their fragmentary condition, and partly to imperfect observations as to their exact stratigraphical position, gives to this class of evidence a tinge of uncertainty. Hence such materials

are more liable to the attacks of opponents; but after all deductions are made on the plea of "not proven," there remains a residuum of irrefragable data which so far support the theory of evolution as applied to Man. While no part of the skeleton is without some measure of determinative value, the skull is of special importance, because it is of itself sufficient to supply the principal elements of the distinction between the human races. Between forty and fifty human skulls, more or less intact, and supposed to date back to Quaternary times, have been found in almost as many different localities throughout Europe, occasionally in alluvial deposits, but more frequently in the accumulated débris of caves and rock-shelters. Some years ago MM. Hamy and De Quaterfages carefully examined all the fossil remains of the Quaternary population of Europe then known, and classified them under the names of the localities where the most typical specimens were found. Among dolichocephalic, or long-headed, they recognised two distinct races, one represented by a skull found at Canstadt, near Stuttgart, and the other by a skull from the rock-shelter of Cromagnon, in the Dordogne district. The brachycephalic, or broad-headed, are made to represent four races, under the generic designation of Furfooz, the name of a cave in the valley of the Lesse, in Belgium.

| | | |
|---|---|---|
| 1. The race of Canstadt, Cephalic index, | | 72 |
| 2. The race of Cromagnon, Cephalic index, | | 73·76 |
| 3. The races of Furfooz, | 1st Furfooz, Cephalic index, | 79·31 |
| | 2d Furfooz, do. | 81·39 |
| | Grenelle, do. | 83·53 |
| | La Tanchère, do. | 84·32 |

Under this fanciful nomenclature all the supposed Quaternary skulls collected to date were classified, each in accordance with the type to which its osteological characters most nearly conformed. Thus, under the so-called Canstadt race, we have a number of well-known skulls, such as that famous specimen from the Neanderthal cave, near Dusseldorf, that of "the fossil man of Denise," and others from widely separated localities, as Eguisheim in the Upper Rhine district, Brux in Bohemia, Olmo in the valley of the Arno, near Florence, &c. This type of skull is characterised by being extremely dolichocephalic, and having a low retreating forehead and very prominent superciliary ridges. On extending the area of observation, it soon became apparent that this form of skull was not confined to the Quaternary period, but occupied, in subsequent ages, even a wider geographical distribution. It has been found not only in caves but in the dolmens, Gallo-Roman cemeteries, and various tombs, both ancient and modern, from Scandinavia to Spain, and from Iceland to the Crimea and other parts of Russia. Nay, more, men with heads of the Canstadt type may be seen stalking among the present day philosophers. Let me just quote the following remarks by De Quaterfages on this point:—

"At the Paris Congress, M. Vogt quoted the example of one of his friends, Dr Emmayer, whose cranium exactly recalls that of Neanderthal, and who is nevertheless a highly distinguished lunacy doctor. In passing through the Copenhagen Museum, I was struck by the Neanderthal characters presented by one of the crania in the collection: it proved to be that of Kay Lykke, a Danish gentleman, who played some part in the political affairs of the seventeenth century. M. Godron has published the drawing of the skull of Saint Mansuy, Bishop of Toul in the fourth century, and his head even exaggerates some of the most striking features of the Neanderthal cranium. The forehead is still more receding, the vault more depressed, and the head so long that the cephalic index is 69·41. Lastly, the skull of Bruce, the Scotch hero, is also a reproduction of the Canstadt type."—(*The Human Species*, p. 309.)

The Cromagnon and Furfooz types have an equally wide distribution, both in space and time. With such diversity in the osteological characters of fossil crania, it is clear that the scientific value of the evidence they are capable of furnishing can only be correctly interpreted when supplemented by collateral sources of investigation. The mere measurement of skulls seems only to prove that the earliest population of Europe showed as great a mixture and diversity of races as are to be found at the present day. I will, however, recur to this subject after disposing of the two remaining heads under which I have classified anthropological materials.

(5) *The Handicraft Products of Man.*—Under this heading we have to deal with a class of evidence unique in nature, and exclusively applicable to Man, as being the only known toolmaker in the world. Although many of the other animals are superior to

him in bodily strength, and are possessed of acuter senses, yet he has succeeded in getting the mastery of them all by the simple invention of manufacturing implements and weapons. Since he attained this art he has, to a certain extent, divested himself of the means of attack and defence with which nature originally endowed him, and he has substituted, instead of them, a system of armour founded upon practices and methods never before used by any other being in the history of the organic world. In swimming, flying, running, &c., Man is nowhere among thousands of competitors. Yet he beats them all in the actual attainment of locomotion by sea or land. Whenever an enemy becomes unmasked, it is sure to succumb eventually to his artifices. The bigger and stronger his antagonist, the more readily does he fall a prey to his cunning and ingenuity. After extinguishing the great giants of the antediluvian world, it would appear that at the present time his greatest opponents are micro-organisms, which, in the form of parasitic germs, establish themselves as colonies in his body, where they consume his very vitals, and in this way bring about his downfall. But he is on their track ; and, as we are told that the resources of civilisation are not yet exhausted, it is to be hoped that he will soon be able to reckon them also among his beaten foes. One great characteristic of man's handiworks is that they bear the impress of intelligence. Hence a specimen of his workmanship, whatever its age, always conveys to the critical eye some knowledge of the technical skill and mental qualities of its maker. Wherever such objects are found, and to whatever period they may belong, it follows to a certainty that their manufacturers were there also. On the supposition that the reasoning power, and its counterpart the manipulative skill of Man, were feeble at first, but improved gradually, we naturally expect that stray objects left behind him at successive stages would disclose indications of his upward progress. This is exactly what we find to be the case. This feature is the magic key by which the long-hidden secrets of prehistoric Man are being unlocked. In short, we have in these handicraft works—implements, weapons, ornaments, temples, tombs, houses, &c.—a graduated scale of man's past civilisation and career on the globe. To this generalisation exception may be taken on the ground that we occasionally find evidence of degeneration in the products of civilisation. But these are local and necessary incidents of the competition between rival races. Worked objects of stone, horn, wood or metal, when met with in stratified deposits, serve a similar purpose in Anthropology that fossils do in geology. Let me here observe, however, that although Man is the only being who has acquired and developed the power of fashioning, from the raw material around him, tools and instruments, by means of which he has so largely altered the surface of the globe, and utilised the forces and products of nature in the furtherance of his unique civilisation, he is not alone in the knowledge and application of mechanical contrivances. Many other animals possess, in a minor degree, the power of adapting means to special ends. Results of this principle may be seen in the construction of the dam of a beaver, the nest of a bird, and the cell of a bee. But from all such productions human workmanship is broadly defined by the fact that it involves the use of artificial tools.

One other characteristic feature of man's methods may be noticed. Although fire, in the form of lightning, volcanoes, conflagrations, &c., must have been a conspicuous phenomenon ever since organic life appeared on the globe, he alone has taken advantage of its properties to improve his condition of life. So indispensable to human civilisation has the agency of fire been regarded that the earliest traditions assign its origin to heaven, whence Prometheus is said to have stolen it in a hollow tube. Hence the presence of charcoal in circumstances which preclude its production or importation by cosmical causes—as, for example, when it is met with in the débris of a cave—would be legitimate evidence of the contemporaneity of Man.

(6) *Geology.*—The sciences of geology and anthropology may be said to join hands in the Post-Pliocene or Quaternary period, as the chief problems and phenomena to be investigated are common to both. Between the two sciences there lies a neutral borderland, in which their respective materials overlap and interdigitate in a most remarkable manner. The geologist's chief object is to interpret the life-history of the period; and here, for the first time, he encounters evidence of the existence of Man.

The exceptional combination of climatal conditions which culminated in the glacial period is another strange phenomenon which the geologist is especially called upon to explain; but I believe that among the causes which led to the differentiation of Man from his Tertiary congeners, this Ice age, with the concomitant alteration in climate, will be found to have been an important factor. The gradual interposition of such a huge mass of ice over a large portion of Europe—thus changing a subtropical climate to one of arctic severity—was followed by representatives of the flora and fauna of northern regions; and it would appear that a wide zone in Central Europe became a common habitat for two distinct faunas—one hailing from the north and the other from the south. It is difficult to account for the precise conditions which led to the intermingling of such different species as the mammoth, rhinoceros, Irish elk, cave-bear, cave-tiger, hyena, reindeer, hippopotamus, horse, &c. But whatever may have been the true explanation, whether inter-glacial genial periods, or great extremes of temperature in the summers and winters, or any other cause, it is certain that a succession, or successions, of such climatal alterations taxed the life-capacity and power of endurance of these animals to a degree which ultimately became unbearable. Now they are almost all gone from these localities. A number of them have become extinct, and others are still represented in more congenial climates, according as they possessed northern or southern proclivities. Man was the contemporary of them all, and he is the only conspicuous animal which successfully battled against these intensely adverse circumstances. Man has emerged from this singular contest, still bearing traces of the means to which he resorted in the struggle for life. An upright posture, a manipulative hand, and a highly reflective brain are trophies of which he may be justly proud; but, like scars, they tell a tale of many battles. The history of these departed mammals, among which Man in his youthful days lived, moved, and had his being, throws much light on the ways and methods by which he accommodated himself to the exigencies of the climatal instability which obtained in Quaternary times.

But, besides these common interests, the anthropologist is largely dependent on the geologist for explanatory details of the phenomena with which he has to deal, such as the position and chronological sequence of river gravels, sea beaches, aqueous deposits, peat beds, the formation and filling up of caves, &c., in all of which relics of

Man are most commonly met with. The nature of the matrix in which a worked object is found, its depth below the surface, the composition and disposition of the superincumbent débris, &c., are also problems to be decided by geological skill.

On the borderland of the science many other issues fall to be determined by collateral evidence: for example, a most legitimate inference from such a discovery as the skeleton of a reindeer with a stone axe embedded in its skull would be that this animal and Man were contemporary. If the supposed discovery were in Lapland, where the reindeer still lives, its archæological value would be almost *nil;* on the other hand, if it had been in one of the Dordogne caves, it would reveal an important fact, viz., that Man lived at a time when the climate in that part of France had been so cold as to permit of the growth of the plants and lichens which form the natural food-supply of the reindeer.

In the preceding remarks I have very briefly described some of the scientific facts and speculations which loomed on the philosophical horizon when the theory of the natural development of Man was first promulgated; and I have also given a few illustrations of the nature and sources of the evidence advanced in support of it. To complete the sketch, I now proceed to inquire what progress this theory has made among thoughtful men,—I say, thoughtful men; because it would be useless to appeal to the unthinking masses for a verdict on a doctrine involving so many difficult and abstruse problems. In this country no special school for the study of Anthropology has yet been founded, but in France there is l'Ecole d'Anthropologie, with a staff of twelve Professors, who apportion the subject as follows:—

| | |
|---|---|
| Géographie médicale. | Histoire des civilisations. |
| Anthropologie pathologique. | Anthropologie zoologique. |
| Anthropogénie et embryologie. | Anthropologie physiologique. |
| Ethnologie. | Ethnographie comparée. |
| Anthropologie biologique. | Anthropologie préhistorique. |
| Linguistique et ethnographie. | Anthropologie géographique. |

To form an estimate of the progress made in a science, having such extensive ramifications, belongs to specialists in their respective departments. To attempt to gather up the progressive increments of knowledge garnered in them all during the last quarter of a century, at the end of a single address, would be sheer folly. My remarks will therefore be restricted to a few points in the general aspect of Anthropology, in harmony with those I have already ventured to lay before you.

The tendency to assign strange and apparently unaccountable phenomena to supernatural causes appears to have been a feature common to all past civilisations. To this category were relegated, in early historical times, many of the industrial products of the previous and less civilised races. The Greeks and Romans took particular notice of the polished stone hatchets which were then, as now, occasionally picked up in the fields and other odd places. Unable to account for their production on any other hypothesis, they regarded them as thunderbolts (*ceraunia*), and professed to find them wherever lightning was seen to strike the earth; and hence they came to be used as charms and talismans, to which extraordinary virtues were attributed. Some variant of the popular belief so long prevalent in this country, that flint arrow-heads were the missiles of elves or fairies, was widely spread throughout the world. Equally persistent and widespread was the idea that these stone objects were possessed of the property of healing diseases and averting threatened calamities, such as the evil-eye and other imaginary ills. Dr Bellucci, of Perugia, in his well-known *Catalogue of Italian Amulets*, has tabulated, under the heading *Pierres de foudre*, twenty arrow-heads and thirty stone axes which had been used as charms throughout the country. Among the curiosities imported into Europe, after the geographical discoveries of the 15th century had opened up the New World to research, were stone implements, such as axes, chisels, arrow-points, knives, &c., found actually in use among various primitive people. This was the first clue to the true function of the so-called *Ceraunia* and *Pierres de foudre* of the ancients. In 1723 we find Jussieu suggesting, at the *Academie des Sciences*, that the *Pierres de foudre* were the implements of a savage people who lived in Europe in earlier times. But it remained to the newborn science of Anthropology to give the *coup de grace* to this kind of superstition.

Another of its more immediate results was a complete explanation of the curious custom which preserved the use of stone weapons in religious ceremonies, long after the discovery of metals had superseded them in the ordinary affairs of life. For example, in the Egyptian process of embalming, the first incision on the body was made with a knife of Ethiopian stone, no doubt flint, as many such implements, supposed to have been used for this purpose, have been found in the tombs and elsewhere throughout the country. The Jews used stone knives for performing the ceremony of circumcision; and the priests of Baal when, as on the occasion of high festivals, they hacked their persons in order to ingratiate themselves with their god. It was a flint knife that Hannibal used when he sacrificed a lamb before he gave battle to Scipio on the banks of the Ticino. Underlying this religious conservatism, which ultimately gave a sacred character to these implements, was the fact that they were survivals of an age when metals were entirely unknown. This ceremonious retention of them to later ages may be paralleled by the present-day custom of placing an urn on the top of a sepulchral monument.

The light thrown upon the past by the correct interpretation of these worked stone objects, and the recognition of the ruder flint implements as the work of Man, opened up a novel field of research. The work of collecting and classifying specimens has progressed steadily ever since, and there is now at the disposal of archæologists a vast amount of such material. The principle of classifying stone implements into Palæolithic and Neolithic, first suggested by Sir John Lubbock, depends on whether they are roughly chipped or polished. The idea is that before Man recognised or acquired the art of giving a sharp edge to his cutting tools, he went on for ages manufacturing them by the rough and ready process of chipping. However trivial the new feature of polishing may be regarded at first sight, it really marks an important stage in the progress of civilisation.

In the workmanship of palæolithic objects various degrees of skill are detected, which may be traced in descending order to the stage when it becomes difficult to say whether we are dealing with the handicraft products of Man or not. They have indeed disclosed to the eye of the expert so many distinct phases as regards form, size, manner of chipping, and patina or surface lustre, that their classification is by no means an easy task. French anthropologists divide them into four divisions, representing as many progressive epochs, under the names of the localities which have yielded the most characteristic specimens, viz., Magdalénien, Solutréen, Moustérien, and Chelléen. The area of their distribution is rapidly extending, and now embraces not only numerous localities throughout Europe, but several in Asia, Africa, and North America. But the special *technique* in their manufacture, though clearly showing a progressive skill, is of little value in deciding the question of their antiquity, as it might have been acquired in a few hundred years instead of as many centuries or even more. Hence, to assign a more definite meaning to the duration of the series of changes in this chronological sequence, the anthropologist has recourse to collateral sources of information. And this is one of the points in which the geologist comes so opportunely to his assistance. The latter takes no cognisance of the objects themselves, but examines the gravel beds, or other localities, in which they were found, and so, on geological grounds, assigns to them a relative antiquity. Here, he comes to a bed of ancient river-drift, left high and dry many feet above the present highest flood-mark of the river, there, but still higher up the slope, another similar bed, both of which yield flint implements. These two gravel deposits are, of course, of different ages ; and so the geologist also comes to form a chronological scale based on the time the water has taken to excavate the valley. This is how Sir John Evans depicts the chronological element involved in this problem:—"Taking our stand on the high terraces at Ealing, or Acton, or Highbury, and looking over the broad valley, 4 miles in width, with the river flowing through it at a depth of about 100 feet below its former bed, in which, beneath our feet, are relics of human art deposited at the same time as the gravels, which of us can picture to himself the lapse of time represented by the excavation of a valley on such a scale by a river greater, perhaps, in volume than the Thames, but still draining only the same tract of country?

"But when we remember that the traditions of the mighty and historic city now extending across the valley do not carry us back even to the close of that period of many centuries when a bronze-using people occupied this island; when we bear in mind that beyond that period lies another of probably far longer duration, when our barbaric predecessors sometimes polished their stone implements, but were still unacquainted with the use of metallic tools; when to the Historic, Bronze, and Neolithic Ages we mentally add that long series of years which must have been required for the old fauna, with the mammoth and rhinoceros, and other, to us, strange and unaccustomed forms, to be supplanted by a group of animals more closely resembling those of the present day; and when, remembering all this, we realise the fact that all these vast periods of years have intervened since the completion of the excavation of the valley and the close of the Palæolithic Period, the mind is almost lost in amazement at the vista of antiquity displayed."—(*Ancient Stone Implements*, &c., p. 622.)

As already mentioned, the contents of caves sometimes afford the means of estimating the relative sequence of events during Quaternary and recent times. As an illustration, we may again refer to the singularly suggestive phenomena disclosed by the now completed excavation of Kent's Cavern. No investigation has ever been conducted under more qualified auspices, nor with greater care, than the excavation of this cave; and consequently the results are correspondingly valuable. Briefly stated, the following deposits were uniformly met with from above downwards:—

(1) Black mould, 3 to 12 inches thick.
(2) A layer of granular stalagmite, 1 to 3 feet thick.
(3) Cave earth, of variable depth.

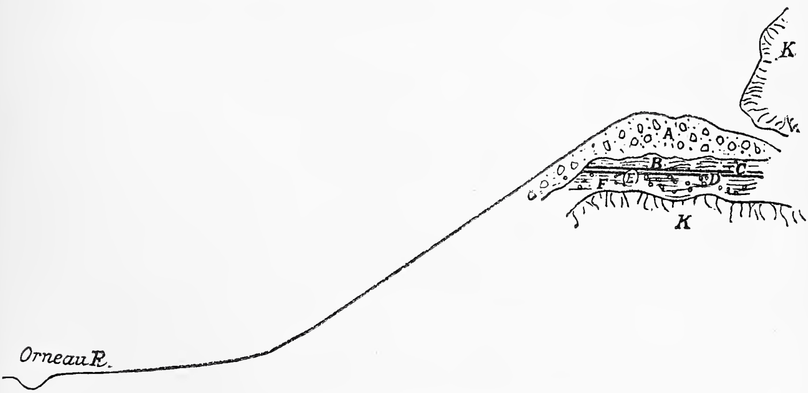
Upon examining these three deposits, it was found that the upper contained relics of modern Man associated with a fauna essentially the same as that of the present day, and representing a period of at least 2000 years. The bed of stalagmite, which contained few relics of any kind, formed a complete partition between the two deposits above and below it, and virtually separated the remains of two totally distinct civilisations. The contents of the cave-earth below it included implements and tools of flint and bone, shells of pectens, ashes, and charcoal, together with the broken bones of a variety of animals. But not only were these worked objects of palæolithic types, but the bones represented, for the most part, an altogether different fauna. Not a bone of the ox, sheep, goat, pig, dog, &c., animals exclusively encountered in the deposits above the stalagmite; but instead of them there were the bones of the cave-lion, cave-hyena, mammoth, woolly rhinoceros, wild bull, Irish elk, reindeer, grizzly bear, wild-cat, horse, beaver, &c.

These records give us some strange glimpses of past humanity in the south of England. They prove that palæolithic Man frequented this cavern as a hunter of the great extinct mammals which formerly roamed over the country. Then, for some reason or other, probably of a climatal nature, came the Stalagmitic period, during which the cave was seldom frequented by Man or animals. How long this continued it is impossible to say; but when the conditions which induced it passed away, Man and the surrounding animals again resorted for shelter to its gloomy recesses. During the deposition of the stalagmite, the records of the cavern are almost silent as to what was going on outside. When they are again resumed, how different is the tale they tell. Everything, man and beast, is changed! Were Palæolithic Man to reappear on the scene, he would hardly recognise his own kindred amidst the luxuries of the Bronze age. But the greatest and saddest change to him would be in the animal world; and with the disappearance of the mammoth, the great Irish elk, and other big game with which he was familiar, he would probably think that life was not worth living in such degenerate days.

Among the so-called fossil remains of Man those known as "les hommes de Spy" appear to me to be the most important. In 1886 two skeletons were found deeply buried in undisturbed débris at the entrance to a grotto called Belche-aux-Roches at Spy-sur-l'Orneau, in the province of Namur, Belgium. The interior of the grotto had been examined more than once, but in front of it there was a terrace, projecting 13 yards, which had not been previously excavated. It was in this terrace that MM. Lohest and De Puydt made excavations which unearthed the skeletons. The outer skeleton was found at a distance of 26 feet from the entrance to the cave, under a mass of rubbish 12 feet 6 inches in depth, and composed of four distinct strata, none of which appeared to have been hitherto broken through. It lay on the right side, across the axis of the cave, with the hand resting on the lower jaw, and the head towards the east. The other skeleton was 8 feet nearer the present entrance to the cave, but its position was not determined with so much accuracy as the first named. Associated with these skeletons, *i.e.*, on the same stratum, were worked flints of the type known as Moustérien, and some animal remains representing the following fauna :—

Rhinoceros tichorhinus (abondant).
Equus caballus (très abondant).
Cervus elaphus (rare).
Cervus tarandus (très rare).
Bos primigenius (assez abondant).
Elephas primigenius (abondant).
Ursus spelæus (rare).
Meles taxus (rare).
Hyena spelæa (abondante).
(*Congrès International, &c., Paris*, 1889, p. 322.)

Immediately above the skeletons was a hardened layer composed of chippings of ivory and flint, pieces of charcoal, and some angular stones of the surrounding limestone rock. Above this there was a reddish deposit containing the remains of the same fauna, but the worked objects indicated a decided advance in civilisation—awls




and borers of flint; needles, beads, and ornaments of bone and ivory. Over this came a bed of yellowish clay, in which were still found bones of the mammoth as well as flint implements. And, finally, there was a mass of clay and fallen rocks, without relics of any kind (see section).

The possibility of these bodies being brought here and buried in graves dug for the purpose is, according to MM. Fraipont and Lohest, inadmissible. This is how they account for them. "L'interprétation les plus logique, au contraire qu'il soit permis de donner à la coupe constatée, est que les hommes de Spy sont morts à l'entrée de la grotte qui leur avait servi de demeure, sur le sol qu'ils avaient en partie contribué à former par leurs débris de cuisine" (*Archives de biologie de Gand*, 1886, p. 668).

The osteological characters of one of the Spy crania correspond in a remarkable degree with those of the Neanderthal skull so frequently discussed by anthropologists (see Plate). Here are tabulated a few of Professor Fraipont's measurements :*—

| | Spy. | Neanderthal. |
|---|---|---|
| | mm. | mm. |
| Antero-posterior diameter (max.), | 200 | 200 |
| Transverse do. do. | 140 | 144 |
| Frontal (min.), | 104 | 106 |
| Do. (max.), | 114 | 122 |
| Horizontal circumference, | 580 | 590 (571)? |
| Cephalic index, | 70 | 72 |

As regards the great development of the superciliary prominences, the low retreating forehead, the depressed and elongated form of the cranium, both these skulls present a more brutal appearance than any human skull known. The fragmentary condition of the Neanderthal skull prevents us carrying the comparison between these two early specimens of humanity further. The Spy skull is, however, not only more perfect, but it was associated with nearly the whole skeleton, and, according to Professor Fraipont, its entire anatomical characters bear out the same lowness of type. The jaws are deep and powerful, the chin slopes away from the teeth downwards and backwards, and the teeth and alveolar processes have a striking prognathic appearance. The long bones, also, differ materially from those of the normal Belgians of the present day, being generally shorter and stouter. The bones of the thigh and forearm have a curiously-bent appearance, and the lower ends of the former are so fashioned as to prevent the limb being fully straightened. It is, however, only just to say that, so far as the measurements of

* *Congrès International, &c.*, 1889, p. 333.

the other Spy skull could be determined, its Pithecoid characters are less pronounced. The cranial vault is more lofty, and the cephalic index at least 74.

The Belgian Professor comes to the conclusion that the Spy men belonged to a race relatively of small stature, analogous to the modern Laplanders, having voluminous heads, massive bodies, short arms, and bent legs. They led a sedentary life, frequented caves, manufactured flint implements after the type known as Moustérien, and were contemporary with the mammoth and tichorrhine rhinoceros.

On the supposition that Man is descended from one of the higher vertebrates by a process of natural development, it necessarily follows that he must have passed through a series of physical changes which connected him with the generic stock by a continuous chain. Hence one of the primary problems which anthropologists had to consider was to ascertain if these connecting links had left any traces behind them which could throw light on the remarkable transformation he had undergone. It is for this reason that the remains of fossil Man have occupied such a prominent place in these discussions. But no sooner had a fair start been made in this investigation than the entire class of evidence became partly discredited by the eagerness of its own votaries. Not content to rear, slowly and cautiously, a substantial structure on a basis of solid facts, they, in their haste, admitted into their argument materials of a more or less problematical character, which, when subjected to the strain of criticism, at once gave way. To this category must be assigned the facts hitherto advanced in support of the existence of Tertiary Man—a question which has so largely occupied the attention of French anthropologists. M. Mortillet devotes not less than a sixth part of his book, *Le Prehistorique*, to the consideration of "l'homme tertiare," and goes so far as to give him a generic name (*Anthropopithecus*). But it is unnecessary to analyse the facts adduced to prove the existence of this "precurseur de l'homme," as their argumentative value is questionable. Indeed, Mortillet acknowledges that it is by pure reasoning he has arrived at this conclusion; but, for this very reason, he ranks it among the greatest discoveries of the age, and exclaims:—"Cela rappelle Leverrier découvrant, sans instrument, rien que par le calcul, une planète. Cela rappelle les linguistes découvrant aussi les Aryens rien que par des données de linguistique" (*Le Prehistorique*, p. 104).

The only fact bearing on the probable origin of Man in the Tertiary period which strikes me as worth mentioning is that, in cranial development, the Simian races of to-day appear to have made no advance on those of the Pliocene period. This has been shown by the facial and cranial characters of *Mesopithecus Pentelici*, found at Pikermi, at the foot of Mount Pentelicus, in Greece. The lower jaw of *Dryopithecus Fontani*, also, has a less Simian form, and approaches more to that of Man than those of the present anthropoid apes. This is what might be expected, as, between Man and the higher apes, there is no field of existence for an intermediate being. He must compete either on brute principles or on those evolved by human ingenuity. Since Man discovered, and rapidly monopolised, the principles of intelligence, there was only one platform for the successful struggle for existence; and during this period, not only have apes remained stationary, or perhaps retrograded, but many of the less progressive human races have fallen into the background and died out. Thus the gap between civilised Man and brute creation has widened at both ends by the progressive development of the former, on the one hand, and the degeneration of the latter, on the other. The demand to produce the "missing links" of this transformation derives its plausibility from ignorance. That any trace of such links would remain to this day is due to a mere accident in nature. How rarely do the conditions occur which preserve the body of a land animal for centuries; and should they occasionally take place, how small is the chance of finding its fossilised remains at the present time. Fossil monkeys have been found in considerable numbers in Miocene and Pliocene deposits in Greece, France, and England. This indicates that they were comparatively numerous in these ages, and that the climate was then much milder than that which now obtains in the corresponding latitudes in Europe.

Palæontological researches have not sensibly altered the question of man's relationship with the lower animals since 1864, when Prof. Huxley summed up the problem as follows:—"That the Neanderthal skull exhibits the lowest type of human cranium at present known, so far as it presents certain pithecoid characters in a more exaggerated form than any other; but that, inasmuch as a complete series of gradations can be found among recent human skulls, between it and the best developed forms, there is no ground for separating its possessor specifically, still less generically, from *Homo sapiens.* At present, we have no sufficient warranty for declaring it to be either the type of a distinct race, or a member of any existing one; nor do the anatomical characters of the skull justify any conclusion as to the age to which it belongs."—(*Natural History Review*, 1864, p. 443.)

But the difficulty of discovering and correctly interpreting the phenomena of fossil Man is a poor apology for the readiness with which anthropologists have admitted into their speculations so many doubtful data. As a final touch to the disputations of earlier years, in regard to the supposed Simian characters of the Neanderthal and Canstadt skulls, I may quote the following remarks by Professor Virchow, announcing the conclusion to which a congress of anthropologists, held last year at Ulm, came to in regard to these two skulls:—

"Les objets de la paléo-anthropologie sont si rares et pour la plupart si douteux que jusq'ici la tentative de la description de la race la plus ancienne de l'homme quaternaire dépasse les forces de la science. En Europe, nous avons en deux exemples bien décourageants; ceux du crâne de Canstatt et du crâne du Néanderthal, qui ont été regardés par des savants éminents comme ayant appartenu aux ancêtres directs de la race Européenne primitive. Il y a quinze jours, au Congrès de anthropologues allemands, à Ulm, nous avons discuté la question soulevée a propos de ces deux pièces, et nous avons trouvé que le crâne de Canstatt n'appartient pas à l'époque quaternaire et que le crâne de Néanderthal est pour le moins très loin d'avoir une forme typique" (*Congrès International, &c., à Moscow*, 1892, vol. ii. p. 224).

On what grounds the Ulm anthropologists founded their objections I do not know, but it seems to me that it was in defiance of all scientific methods and rules of correct reasoning the Canstadt skull had ever been adopted as a type of a fossil race. The facts of its discovery are as follows:—In the year 1700 the then Duke of Wurtemberg excavated a Roman oppidum, in the neighbourhood of Stuttgart, in the course of which a large quantity of bones, including those of Quaternary animals, were dug up and preserved in the Duke's museum. A hundred years later a human jaw was found among these bones, and on this discovery being brought under the notice of Cuvier he declined to regard it as of any value, owing to the entire absence of information as to its position in the earth. In 1835 Mr Jaeger found in the same collection portion of the cranial vault of a man, and brought it forward as an argument in favour of the coexistence of Man with the great extinct mammals.

Sir Charles Lyell accepted much of the speculations founded on such evidence. Nor, indeed, is this the only case in which his accuracy has been called in question. A human jaw found by Professor Crahay, near Maestricht, and known as the "Smeermaas mâchoire," was described by Sir Charles as coeval with a mammoth tusk disinterred "6 yards removed from the human jaw in horizontal distance" (*Antiquity of Man*, 3rd ed., p. 339). Now, however, it is proved that the tusk was 24 feet deeper than the skull, and that the latter was merely a relic from a crannog of the Neolithic period since discovered and investigated. An epitome of the evidence on which this prosaic conclusion has been arrived at will be found in my work on the *Lake-Dwellings of Europe*, pp. 305–6.

I cannot close this sketch without mentioning one or two of the more important researches which have enriched the science of Anthropology since the publication of the works of Darwin and Lyell, chief among which are the three following:—

(1) The complete exploration of Kent's Cavern, under the superintendence of Mr Pengelly and a scientific committee of the British Association. The investigation was begun on the 28th March 1865, and continued without interruption to 19th June 1880, at an expense of £1963.—(*British Association Report*, 1883, p. 556.)

(2) The exploration of a series of caverns, some sixty in number, in the valley of the Lesse, near Dinant, and other localities in the vicinity of Namur, Belgium, under M. E. Dupont, director of the Royal Museum of Natural History of Brussels. The excavations were begun in 1864, and continued for seven years, during which time an enormous quantity of the remains of Man and his contemporary

animals in prehistoric times was accumulated.—(*Les Temps Prehistoriques en Belgique, par E. Dupont.*)

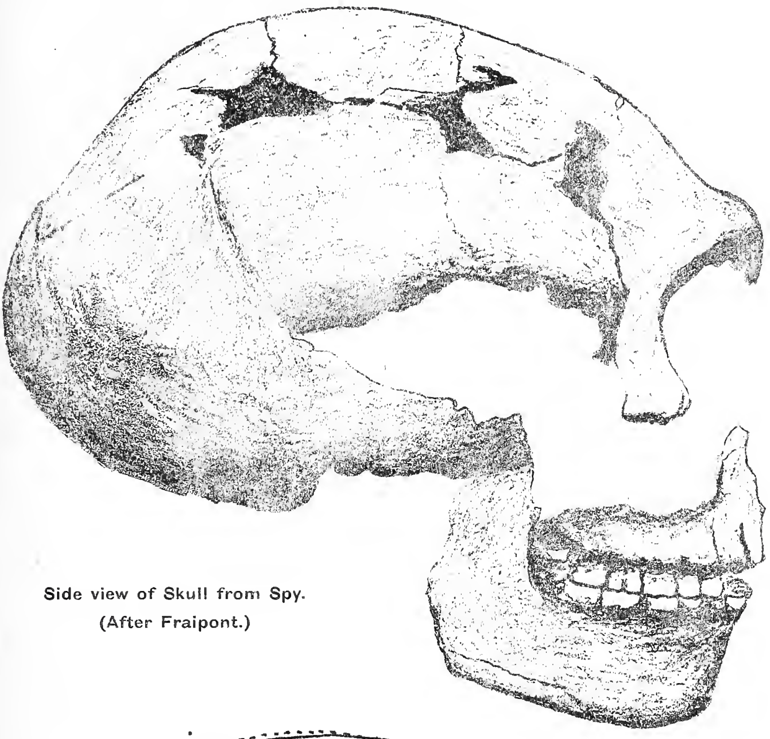
(3) The exploration of a number of caves and rock-shelters in the valley of the Vézère, in the Dordogne district, France, by Messrs Christy and Lartet. The result of their investigations was published in a large quarto volume, entitled *Reliquiæ Aquitanicæ* (1865–75), and revealed a unique phase of human civilisation. Ignorant of agriculture and the ceramic art, and without domestic animals, not even the dog, these Dordogne cave-men successfully hunted the reindeer, mammoth, and other wild animals, with only such weapons as they could manufacture of flint and bone. But, what is most remarkable, under these circumstances they developed a wonderful taste for art, and left behind them a collection of sculptures and engravings which, for spirit and artistic effect, would not disgrace our modern Landseers. By a few scratches on bone and ivory, they faithfully depicted the characteristic features of the animals, hunting scenes, and industries among which they lived. The handles of their poignards and other objects were ornamented, not only with geometrical and bizarre figures of straight, curved, and zigzag lines, but sometimes carved to represent animals. In this kind of sculpturing they displayed great ingenuity in adapting the material at their disposal to the production of a fantastic piece of art, but which always delineated some characteristic trait of the animal represented. The staple food of these troglodytes was the reindeer, and hence the time in which they lived is often known as the Reindeer period (Magdalénien). Characteristic remains of this civilisation have been found in numerous localities throughout Central Europe, especially the South of France. Among the more important collections illustrative of this unique art, in addition to those already mentioned, are those of *Vicomte de Lastic Saint-Jal*, from the rock-shelter of Bruniquel (Tarn et Garonne); of *M. Elie Massénat*, from the Dordogne district; and of *M. Ed. Piette*, from the caves of Arudy and Mas d'Azil (Ariège),—all of which were exhibited at the Paris Exhibition of 1889. The stations at Lortet (Haute-Pyrenées), Montgaudier (Charente), Grotte de Reilhac (Causses du Lot), Thaïngen (Switzerland), and others, have also yielded some further specimens of exceptional designs.

While the flora of their environments is scarcely represented at all by those artists of the Reindeer period, the fauna is depicted in endless variety, over 300 specimens having now been collected—representing man, mammoth, reindeer, auroch, horse, bull, wild-goat, saïga, bear, salmon and other kinds of fish. It is also remarkable that although their geometrical figures are extremely varied, not a single circle has yet been observed among them.

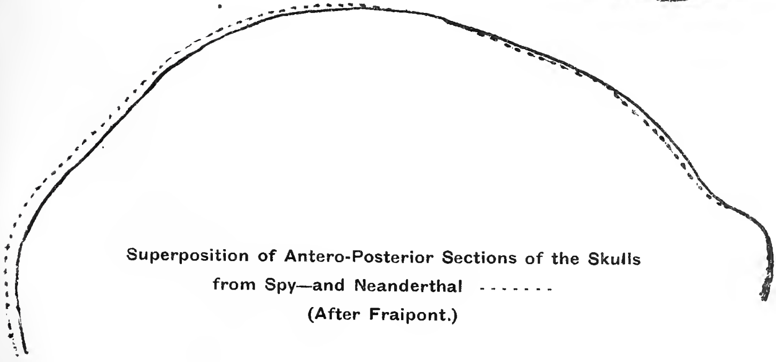
Of singular interest are a few representations of men and women, all of which are nude, although a female seems to be adorned with bracelets and a necklace (Coll. Piette). This figure comes from Laugerie Basse, as well as another, of the same sex, sculptured in ivory (Coll. de Vibraye), and that of a man creeping on his belly, and in the act of hurling a spear at an auroch (Coll. Massénat). Another figure, engraved on portion of a *bâton de commandement*, from La Madeleine (*Musée de St Germain*), represents a man walking, and carrying a club on his right shoulder.

In conclusion, let me say that these cursory remarks give but a faint idea of the interesting and profound problems embraced by the science of Anthropology. The earlier portion of the period covered by them is destined to be for ever memorable in the history of mankind and civilisation. Never, since the material world became an object of human study and reflection, has there been accomplished, in so short a time, such a complete and far-reaching revolution in current philosophical opinion—a revolution whose effects are not confined to Anthropology alone, but permeate every department of knowledge. From the standpoint of evolution, the entire organic world reveals a unity, a harmony, and a grandeur never before reached under any system of speculative philosophy.

DR. MUNRO ON THE RISE AND PROGRESS OF ANTHROPOLOGY.



Side view of Skull from Spy.
(After Fraipont.)



Superposition of Antero-Posterior Sections of the Skulls
from Spy—and Neanderthal - - - - - - -
(After Fraipont.)

A. RITCHIE & SON EDIN^R

## On the Compressibility of Fluids. By Prof. Tait.

(Read Jan. 15, 1894.)

(*Abstract.*)

The recent publication of the full results of Amagat's magnificent experiments has led me to make further comparisons with the empirical formula (originally suggested by the graphs of my *Challenger* work) which I have on several occasions brought before the Society :—viz.

$$\frac{v_0 - v}{pv_0} = \frac{e}{\Pi + p}.$$

I find that Amagat's results, for a number of common liquids, from 1 to 3000 atm. may be *fairly* represented by substituting the following values of $e$ and $\Pi$ in the above formula :—

| | 0° | 10° | 20° | 30° | 40° C. |
|---|---|---|---|---|---|
| Ether . . . | 0·291 | ·296 | ·302 | ·310 | ·319 |
| | 2420 | 2240 | 2100 | 1980 | 1860 |
| Ethylic Alcohol . | 0·274 | ·280 | ·281 | ·287 | ·288 |
| | 3230 | 3130 | 2970 | 2865 | 2700 |
| Methylic ,, . | 0·283 | ·290 | ·295 | ·302 | |
| | 3240 | 3180 | 2990 | 2870 | |
| Propylic ,, . | 0·265 | ·271 | ·277 | | ·274 |
| | 3510 | 3390 | 3200 | | 2880 |
| Bisulphide of Carbon | 0·286 | ·286 | ·291 | ·294 | ·299 |
| | 3970 | 3720 | 3560 | 3370 | 3190 |
| Iodide of Ethyl . . | | 0·288 | | ·291 | |
| | | 3570 | | 2920 | |
| Chloride of Phosphorus | | 0·278 | | ·293 | |
| | | 3490 | | 2990 | |
| Acetone . . . | 0·284 | | | ·298 | |
| | 3180 | | | 2570 | |

For the curiously exceptional case of water we have

| 0° C. | 2°·1 | 4°·35 | 6°·85 | 10°·1 | 14°·25 | 20°·4 | 29°·43 | 40°·45 | 48°·85 |
|---|---|---|---|---|---|---|---|---|---|
| 0·303 | ·303 | ·307 | ·311 | ·313 | ·314 | ·314 | ·313 | ·327 | ·323 |
| 5940 | 6030 | 6220 | 6390 | 6560 | 6680 | 6830 | 6940 | 7520 | 7440 |

whence compressibility for low pressures,

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 0·0000511 | 503 | 493 | 486 | 478 | 470 | 459 | 449 | 434 | 434 |

The agreement with the experimental data would be somewhat closer if $\Pi$ for any one temperature were (in accordance with theory) regarded as a quantity which increases with the compression produced.

For the present, as no definite theoretical basis has been assigned for it, the formula must be regarded merely as an exceedingly convenient mode of summarizing the experimental results; justified by the closeness of its general agreement with them.

On these numbers remark

*First,* that $e$ is nearly the same for all the liquids in the table:—its lowest value being for propylic alcohol, and its highest for water. But the differences of these extremes from the mean of all are less than 7 per cent. Hence it seems that ordinary liquids, as a rule, would be reduced by infinite pressure to about 70 per cent. of their usual volume:—provided, of course, that the formula remains applicable for pressures immensely exceeding even the enormous ones applied by Amagat.

*Second,* $e$ increases, as a rule, with rise of temperature. [But it does not appear to increase, in any case, so much as to make the ultimate volume diminish when temperature rises.]

*Third.* Except in the case of water, $\Pi$ falls off rapidly with rise of temperature. This was, of course, to be expected from the increase of volume; and it is the chief cause of the increase of compressibility as given by the formula. But the value of $\Pi$ does not seem to vary inversely as the square of the volume.

*Fourth.* In the exceptional case of water, $\Pi$ increases steadily with rise of temperature, at least up to 40° C. This is the immediate cause of the diminution of compressibility given by the formula as the temperature is raised. But, so far as the present rough calculations go, Amagat's data would seem to make the temperature of minimum compressibility considerably lower than that assigned by Pagliani and Vincentini. [This may be due to the great range of pressure, or to the fact that the formula treats $\Pi$ as a constant instead of taking account of its increase with compression.]

It is interesting to compare, with these, some (necessarily very rough) results for a substance which requires considerable external pressure to keep it in the liquid state. It is shown that if the empirical formula, above, be true generally for any substance, it holds from *any* initial value of $v_0$, provided that we give $e$ and $\Pi$ proper corresponding changes of value. The new Π is greater than the old by the pressure at the new $v_0$. The new $e$ must be employed with the new initial volume to give the ultimate volume. The following data were calculated from Amagat's Tables 13 and 19 (*Ann. de Chimie*, xxix., 1893). The first of the three volumes given for each temperature is that of the substance when just *wholly* liquefied by pressure. This comparison is by no means a fair one, for the range of volumes is very different alike in extent and in situation, for the different temperatures. And, from the extremely great compressibility of the liquid when *just* formed, we should expect to find the assumption of constant Π very far from the truth.

### Carbonic Acid.

| Temp. | 0° C. | | 10° | | 20° | | 30° | |
|---|---|---|---|---|---|---|---|---|
| | *p.* | *v.* | | | | | | |
| | 34·4 | ·002145 | 44·4 | ·002338 | 56·4 | ·002609 | 70·7 | ·003282 |
| | 500 | 1781 | 500 | 1826 | 500 | 1876 | 500 | 1926 |
| | 1000 | 1656 | 1000 | 1685 | 1000 | 1716 | 1000 | 1748 |

From these we obtain the following sets of values of $e$ and Π :—

| | | | |
|---|---|---|---|
| 0·335 | 0·373 | 0·424 | 0·527 |
| 420 | 276 | 170 | 48 |

The value of Π, calculated from the altered formula, has, in each case, been diminished by the corresponding initial value of $p$. We see that $e$ increases with great rapidity as the temperature rises :— but the indicated ultimate volumes of carbonic acid, under infinite pressure, are not much affected thereby, being respectively

0·00143 147 150 155

where the unit is the volume of the gas at 0° C. and 1 atm.

The values of Π are, of course, small; and they diminish rapidly with rise of temperature. [The critical point is about 31°·35 C., which is but little above the highest temperature in the table.]

A fairer test than the above, from one point of view at least, might have been based upon Amagat's important Table 17, had it given data for (say) vol. = ·00225 at each temperature in addition to those at ·0025 and ·0020. I have done the best I could, by taking the nearest data directly given in Table 13. Here are a few of the results obtained.

### Carbonic Acid.

| | 20° C. | | 30° | | 40° | | 50° | |
|---|---|---|---|---|---|---|---|---|
| | $p$. | $v$. | | | | | | |
| | 64·4 | ·0025 | 109 | ·0025 | 155 | ·0025 | 201 | ·0025 |
| | 150 | ·00217$\dot{3}$ | 200 | ·0022 | 225 | ·00228 | 300 | ·002255 |
| | 300 | ·002 | 384 | ·002 | 470·5 | ·002 | 560 | ·002 |
| $e$ | ·2833 | | ·2936 | | ·3136 | | ·3312 | |
| Π | 35·6 | | 22·7 | | 24·5 | | 34·6 | |
| Ult. Vol. | ·001792 | | ·001766 | | ·001716 | | ·001672 | |

Other deductions from Amagat's data are given, in considerable numbers:—from regions of the $CO_2$ diagram in which Π is respectively +, −, or even zero, the latter belonging of course to the conditions under which it behaves as a true gas. Thus, taking the data for volumes

0·01636, 0·013, and 0·01

we obtain the values of Π given in the first line of the table below. Here the substance was, throughout, at density less than the critical. The second line gives the corresponding results for a range of volumes which *includes* the critical volume:—viz.

0·00578, 0·00428, 0·00316.

The application of the formula to this series (where the part of the isothermal which is treated contains a point of contrary flexure) is obviously a matter rather of curiosity than of science.

Finally, the third line gives data for volumes all well under the critical volume:—viz.

0·00316, 0·00250, and 0·002.

### Values of Π for $CO_2$ (in Atmospheres).

| Temp. | 30° C. | 35° | 40° | 50° | 60° | 70° | 80° | 90° | 100° | 198° |
|---|---|---|---|---|---|---|---|---|---|---|
| | (58·5) | | 34·3 | 14·2 | 4·9 | 2·4 | 0·5 | − 1·2 | − 2·1 | − 8· |
| | | −73·5 | −75·5 | −77 | −78·6 | −80·5 | −80· | −81·1 | −80·3 | −80·5 |
| | [35·6] | | −38·6 | −43 | −42 | −46·5 | −47 | −46·5 | −46·5 | |

The single number in ( ) refers to vapour, that in [ ] to liquid; all the others to gas. The results for volumes greater than the critical volume are very interesting.

The rest of the paper deals with (unsuccessful) attempts to apply, to Amagat's data, the equation of Van der Waals :—viz.

$$\left(p+\frac{A}{v^2}\right)(v-\beta)=RT.$$

The arguments in consequence of which the constituent $A/v^2$ was originally introduced and, as I have elsewhere* endeavoured to show, incorrectly introduced, were specially based upon the properties of liquids, rather than of fluids in general; and it is therefore to be expected that the formula, if valid, should be specially applicable to liquids.

The most valuable characteristic of the equation above, in addition to its special merit of giving in certain cases three real values of $v$, and therefore, in a sense, representing the results of Andrews and the conclusions of J. Thomson, is its simplicity. But this simplicity depends essentially upon the understanding that A, $\beta$, and R are genuine *constants;* or, at least, may be treated as such through moderate ranges of volume :—as, for instance, in the compression of an ordinary liquid by 3000 atmospheres. The equation loses its value (from this point of view) entirely if, as has been suggested, $\beta$ is a sort of adjustable constant! For if it be so, it ought to be expressed as a function of $v$, or of $v$ and $t$, and then the simplicity of the whole is gone.

Selecting, as before, a set of three corresponding pairs of values of $p$ and $v$ for any one temperature, we form three equations which lead to a quadratic in A, when $\beta$ and R are eliminated. This involves heavy numerical work, and the results are so much modified by very slight changes in the data (quite within the limits of experimental error) that I was fain to try the simpler process of *assuming* tentative values for A, and determining the other constants from them :—the equations being then linear. But I found that very wide ranges of tentative values of A seemed to suit the conditions, to the same (extremely rough) approximation. I could get nothing satisfactory. The reason is easily found by making a case in which the labour of calculation shall be, to a considerable extent, avoided. It is clear, from the numbers in the early part of

* *Trans. R.S.E.*, xxxvi. ii. (1891). See also Correspondence with Lord Rayleigh and Prof. Korteweg (*Nature*, xliv. and xlv.).

this paper, that we may lawfully assume the existence of a liquid which, for some special (ordinary) temperature, shall give

$$\Pi = 2700 \text{ atm.}, \; e = 0{\cdot}3 .$$

With these numbers the calculation is very much simplified. For such a liquid, if its volume were 1 at atmospheric pressure, would be reduced to 25/28 by 1500 atm., and to 16/19 by 3000 atm. The quadratic to which Van der Waals' formula leads, is found to have imaginary roots!

The main cause of this totally-unexpected result seems to be the factor $1/v^2$ in the term corresponding to K. Its effect is to make K increase at a rate quite inconsistent with the experimental data, at least if the rest of the equation is to retain its present form. This is easily seen by taking the following roughly approximate values of $\frac{dp}{dt}$ for ether, at constant volume, which I obtained by a graphic process from Amagat's Table 29.

| | | | | |
|---|---|---|---|---|
| $v$ | 1 | ·95 | ·9 | ·85 |
| $\left(\frac{dp}{dt}\right)$ $v$ const. | 10 | 12 | 14·5 | 17 |
| $p$ at 0° C. | 1 | 460 | 1250 | 2570 . |

Since Van der Waals' equation gives, for constant volume,

$$\left(\frac{dp}{dt}\right)(v-\beta) = \mathrm{R}$$

we easily find the approximate values

$$\beta = 0{\cdot}63, \quad \mathrm{R} = 3{\cdot}8 ;$$

and the complete formula is something like

$$\left(p + \frac{2804}{v^2}\right)(v - 0{\cdot}63) = 1037 + 3{\cdot}8t,$$

where $t$ is temperature centigrade.

This cannot be very far wrong, so far at least as $\beta$ and R are concerned, for it gives the following calculated values of $\frac{dp}{dt}$ (at the *four* selected volumes above) which are compared with the observed values

| | | | | |
|---|---|---|---|---|
| Obs. | 10 | 12 | 14·5 | 17 |
| Calc. | 10·27 | 11·9 | 14·1 | 17·3 . |

But when we calculate the corresponding pressures and compare them with those observed, we have

| | | | | |
|---|---|---|---|---|
| Obs. | 1 | 460 | 1250 | 2570 |
| Calc. | 1 | 134 | 379 | 833. |

The differences between the numbers in each pair are due to the very rapid increase of the K term in the formula, for moderate diminutions of volume. The following comparison is instructive. The first numbers are calculated on the hypothesis that K is inversely as $v^2$. Those in the second line are the corresponding values of K required to make an approximate agreement between Amagat's data, and the (numerical) formula above:—

| | | | |
|---|---|---|---|
| 2804 | 3107 | 3462 | 3881 |
| 2803 | 2781 | 2591 | 2144. |

Thus the requisite values of K diminish rapidly, instead of increasing, as the compression proceeds. In fact it would seem as if Van der Waals' equation gives impossible roots in precisely that limited region where experiment shows that real ones are to be found. I intend soon to examine the cause of this strange result from a purely mathematical point of view.

## On a New Water-Bottle for Collecting Samples of Sea-Water from Moderate Depths. By H. N. Dickson, F.R.G.S.

(Read May 21, 1894.)

While making physical observations during some recent cruises on board H.M.S. "Jackal," I was on several occasions made to feel the want of a water-bottle which could be relied on to act with precision in rough weather. Mill's slip water-bottle, which leaves nothing to be desired in its working in smooth water, at least up to depths of 400 or 500 fathoms, is apt to be closed prematurely by the jerk on the sounding line when the ship is rolling or pitching in a sea-way; and considerable doubt arises as to the point from which samples are really obtained.

It seemed that the object might be attained, where moderate depths only were concerned, by sending a vessel down *closed*, and opening it at the required depth by means of some modification of the reversing apparatus of the "Scottish" thermometer frame. Until the external pressure becomes sufficient to make the necessary stopcocks leak, there can be no doubt as to the purity of the sample admitted to the bottle, the small volume of compressed air can easily be got rid of, and if all the orifices opening into the bottle point in one direction there is no tendency for the sample to become mixed with other waters in hauling up, even although the stopcocks remain open.

Figs. 1 and 2 represent a small model I have had constructed, which has been found to work satisfactorily at depths up to 40 fathoms, even in a heavy sea. The main axis of the instrument consists of a brass tube TT′, through which the sounding line is rove. A is the cylindrical bottle, closed by taps BB′ at its upper and lower ends. From B′ an indiarubber tube B′C is led upwards and lashed alongside B, the extremities of the two tubes being in the same plane. Two arms attached to the plugs of the taps are connected by a rod DD′; and at D′ is attached a spiral spring G, whose other end is hooked to a "lug" at T′. The releasing arrangement, FE, I was fortunate in having ready to hand in a piece of apparatus devised by Dr Mill for getting the Rung messengers over a knot or short splice in a sounding line, which he was good enough to lend me for application to its own proper purposes during our work. It is essentially the same as the head of

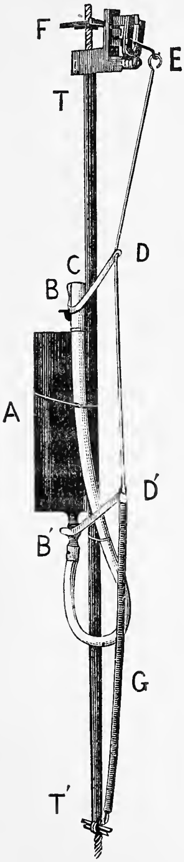



Fig. 1.

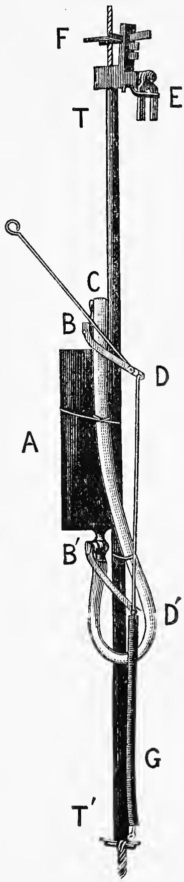



Fig. 2.

the Scottish reversing frame. By the aid of a brass messenger sent down the line, the bracket E is allowed to turn over: to E is attached a small brass hook, which of course turns with it, and which fits into a ring at the upper end of a rod DE, jointed to D.

When the instrument is set, as in fig. 1, the rod DE, hooked to the bracket, extends the spring G and keeps the taps BB′ closed; but as soon as the messenger strikes the arm F, the bracket E turns over, releasing DE. The spring G then contracts, opening the taps and admitting water to A, as in fig. 2. The water enters by the tap B′, through the indiarubber tube B′C, and the air escapes at B. Since B and C are close together in the same plane, little or no change takes place in the contents of A during hauling up.

# Hydrolysis in Some Aqueous Salt Solutions. By James Walker, D.Sc., Ph.D.

(Read May 28, 1894.)

Some years ago, in an investigation on the affinity of organic bases,* I was led to study the amount of decomposition suffered by various salts when dissolved in water, for a knowledge of this amount gives us a method of comparing the relative strengths of weak bases. When these bases are made to unite with a given acid, the extent to which the resulting salts are decomposed, in equivalent aqueous solutions, into acid and base, depends directly on the strength of the various bases. The same process of decomposition by water, or hydrolysis, can also be made use of to determine the strengths of weak acids, and experiments in this direction have been executed by Lellmann and his pupils,† and by Shields.‡ As the amount of free acid or free base in a hydrolysed solution cannot be determined directly by neutralisation, on account of the progressive disturbance of the chemical equilibrium involved in the process, it must be determined by taking advantage of some property of the acid or base which is susceptible of accurate measurement, and does not destroy the chemical equilibrium, or else does so in a manner amenable to calculation.

The properties I employed were the electrolytic conductivity of the solutions and the catalysis of methyl acetate effected by the acid, the speed of this reaction giving a measure of the free acid present. Lellmann, working in aqueo-alcoholic solution, used a colorimetric method, and Shields has employed the saponification of ethyl acetate by the free alkali. In the present paper I communicate the results of a few experiments by Shields' method, made with solutions regarding whose constitution there was some doubt.

## *Solutions of Alkaline Sulphides.*

We very often find it stated in text-books and elsewhere that sodium and potassium hydrosulphides, when mixed in equivalent

* *Zeitschrift für physikalische Chemie*, iv. p. 319.

† Liebig's *Annalen*, cclx. p. 262 ; cclxxiv. p. 121.

‡ *Zeit. physikal. Chem.*, xii. p. 167 ; also *Phil. Mag.* [5] xxxv. p. 365.

proportions with the corresponding hydroxides in aqueous solution, form solutions of the alkaline sulphides; but occasionally this statement is denied on more or less plausible grounds. Mendelejeff, for example, in his *Principles of Chemistry*, writes as follows (vol. ii. p. 207):—"From this [the non-evolution of heat on mixing hydrosulphide with hydroxide] it must not be concluded, as Thomsen concludes, that hydrogen sulphide is a monobasic acid. The impropriety of this conclusion is not only seen from the decomposing action of water on potassium sulphide, but also from the fact that for the formation of this salt there must react $KHS + KHO = K_2S + H_2O$, substances which closely resemble each other, and in such cases hardly any heat is developed,—for example, in double decomposition between salts. The resultant substances, potassium sulphide and water, undoubtedly act on each other, and thus would cause the absorption of heat did not the reaction of potassium hydrosulphide with potassium hydroxide also develop heat." The author here seems to be of somewhat divided mind as to the actual state of an aqueous potassium sulphide solution; but further on, p. 213, he indicates the real explanation:—"Potassium hydroxide, water, and sulphuretted hydrogen form a system whose complex equilibrium is subject to the laws of dissociation, and depends on the relative mass of each substance, on the temperature, and on the dissociation pressure of the component elements." It was with a view to determine this equilibrium in solutions of sodium sulphide and sodium hydrosulphide that the following experiments were made.

Sodium hydrosulphide solution smells strongly of sulphuretted hydrogen at the atmospheric temperature, and, on boiling, easily loses half its sulphur as sulphuretted hydrogen, the sodium sulphide solution thus formed losing sulphuretted hydrogen much more slowly when the boiling is prolonged. At the ordinary temperature a solution of sodium sulphide does not smell of sulphuretted hydrogen, nor do the crystals $Na_2S, 9H_2O$ in perfectly acid-free air, and no indication of the presence of sulphuretted hydrogen is given by lead paper.

To ascertain the effect of temperature, a 20 per cent. solution of the crystalline hydrate, $Na_2S, 9H_2O$, was placed in a flask, and through it there was passed a current of hydrogen previously washed in sodium hydrate solution. The hydrogen was then bubbled through a solution of lead acetate. Not the slightest blackening of the lead solution was observed after a swift current of hydrogen had been passed for four hours at 12°. At 40° blackening was noticeable in about an hour, at 60° in less than half an hour, and at 80° the lead solution began to turn brown almost at once, a distinct black precipitate being deposited after a few minutes.

We may conclude, then, that in fairly dilute solutions of sodium sulphide there is practically no free hydrogen sulphide at the ordinary temperature, whilst in similar solutions of sodium hydrosulphide there is undoubtedly a partial hydrolysis into sodium hydroxide and sulphuretted hydrogen.

The amount of hydrolysis in the hydrosulphide solution must, however, be very small, if we are to judge from its behaviour with regard to ethyl acetate. The action of the hydrosulphide on acetic ether is in itself of some interest, on account of the parallelism we might expect to exist between it and the corresponding hydroxide. We find, however, that in this case, as in so many others, the analogy between sulphur and oxygen is more formal than real: although water and sulphuretted hydrogen are analogous in constitution, their behaviour is as divergent as possible.

Whilst water is one of the most chemically active substances we know, especially in the sense of endowing other substances with reactivity, hydrogen sulphide is not known to be active in this sense at all. The fact of the great activity of water may stand in connection with the circumstance that, according to the electrolytic dissociation hypothesis of Arrhenius, it is formed by the union of the two active ions *par excellence*, namely, $\overset{+}{H}$ and $\overline{OH}$. To the former of these is attributed all acid functions, and to the latter all basic functions. To the ion $\overset{+}{H}$ we have no analogue, but to the ion $\overline{OH}$ there corresponds the ion $\overline{SH}$, which we might not unreasonably expect to be endowed with some of the basic character of $\overline{OH}$. A dilute aqueous solution of a base acts on ethyl acetate in the sense of the following equation:—

$$\cdot\overline{OH} + CH_3\cdot COO\cdot C_2H_5 = C\overline{H_3\cdot COO}\cdot + C_2H_5OH,$$

the corresponding equation for the $\overline{SH}$ ion being

$$\cdot\overline{SH} + CH_3\cdot COOC_2H_5 = C\overline{H_3\cdot COO}\cdot + C_2H_5SH\ ;$$

so that we might expect the formation of ethyl mercaptane by the action of dilute sodium hydrosulphide solution on ethyl acetate. The progress of such an action could be ascertained by titrating with an acid from time to time, an indicator being used which would indicate $\overline{SH}$ but not $\overline{CH_3 \cdot COO}$ ions. After many attempts to find a good indicator fulfilling these conditions, I finally adopted phenacetoline, the end-point with which, although not very sharp, is still sufficiently well defined to show the progress of the reaction.

A solution of sodium hydrosulphide was prepared by dissolving sodium in water and passing sulphuretted hydrogen through the solution of caustic soda thus obtained until no more was absorbed. Excess of sulphuretted hydrogen was removed by a rapid current of hydrogen, and the solution was then brought to decinormal strength. 50 cc. of the solution were mixed with 2·5 cc. of ethyl acetate at 25°, and the mixture was at once titrated with decinormal hydrochloric acid, phenacetoline being used as indicator. No marked diminution of titre was observed until a considerable time had elapsed, as the following table shows:—

| Time from beginning in minutes. | Titre. |
|---|---|
| 1 | 4·75 |
| 12 | 4·63 |
| 66 | 4·55 |
| 1440 | 4·30 |
| 2880 | 3·95 |

After an hour the solution had begun to turn yellow, probably owing to oxidation, and the yellowness increased as time went on, so that no accurate calculations can be based on these numbers. They are sufficient to show, however, that an action corresponding to saponification by an alkali does not take place between ethyl acetate and sodium hydrosulphide; for in the former case the titre of the soda would have disappeared in two or three minutes from the beginning of the reaction under the conditions specified above. They also show that hydrolysis of sodium hydrosulphide into sodium hydroxide and sulphuretted hydrogen takes place only to a very small extent in decinormal solution at 25°, for the rate at which the saponification proceeds is directly dependent on the actual concentration of sodium hydroxide in the solution.

It should be mentioned that, although the solution of sodium hydrosulphide became yellow in the course of an hour when it was mixed with ethyl acetate, by itself it showed no tendency to become coloured even after a week had elapsed.

The following numbers were obtained for the electric conductivity of dilute solutions of sodium hydrosulphide at 25° by Kohlrausch's method.

| Dilution. | Molecular Conductivity. |
|---|---|
| 16 | 96 |
| 32 | 101 |
| 64 | 105 |
| 128 | 107 |
| 256 | 109 |
| 512 | 110 |
| 1024 | 110 |

The numbers given under dilution signify the number of litres of solution which contain a gram-formula-weight of the dissolved substance. The molecular conductivity is expressed on the basis of Siemens' mercury unit of resistance. Ostwald has found that the difference between the molecular conductivity of sodium salts of monobasic acids, at a dilution of 1024 l. and at 32 l., is always very nearly equal to 10. Here we find the difference to be $110 - 101 = 9$, so that in this respect sodium hydrosulphide behaves as the sodium salt of a monobasic acid, and gives no indication of hydrolysis taking place in its solutions to any marked extent.

In the case of the so-called sodium sulphide solutions we have a very different state of matters. Such a solution behaves with respect to rate of saponification and to electrical conductivity precisely as if it consisted of a mixture of equivalent solutions of sodium hydroxide and sodium hydrosulphide. When a saponification experiment was conducted under the same conditions as those given above for sodium hydrosulphide, the rapidity of the action was such that it could not be measured. In order to diminish the rate, a smaller proportion of ethyl acetate was added, and the numbers in the following table obtained. The indicator employed was phenol-phthalein, which gives a sharp end-point when exactly half the sodium in the solution has gone to form sodium chloride with the hydrochloric acid. The only precaution to be observed is to perform the titration rapidly, for the neutral solution obtained becomes alkaline again on standing. 50 cc. of a solution of sodium sulphide (corresponding to 48·5 cc. of decinormal hydrochloric acid, with phenol-phthalein as indicator) were mixed in a thermostat at 25° with 5 cc. of a solution containing 1 cc. of ethyl acetate and 15 cc. of water.

| Time from beginning in minutes. | Titre. |
|---|---|
| 0 | [4·37] |
| 1 | 3·08 |
| 3 | 2·41 |
| 5 | 2·10 |
| 8 | 1·86 |
| 13 | 1·66 |
| 46 | 1·65 |

From these numbers it may be seen that the reaction was completed in 13 minutes: the first titre, given within brackets, was calculated from the composition of the mixed solution. A comparative experiment with an equimolecular solution of sodium hydroxide gave numbers closely corresponding to those in the preceding table. The electrical conductivity of sodium sulphide solutions was measured at 25° with the following results:—

| Dilution. | Molecular Conductivity. |
|---|---|
| 16 | 276 |
| 32 | 287 |
| 64 | 298 |
| 128 | 303 |
| 256 | (298) |

At greater dilutions the molecular conductivity continued to decrease. This is exactly the behaviour of soda solutions, as observed by Kohlrausch, who found a maximum for the molecular conductivity at a dilution of about 166 l. If we assume that the degree of electrolytic dissociation of sodium hydroxide and sodium hydrosulphide is the same in equivalent solutions—as must be very nearly the case, if we judge from the course of the molecular conductivities at the various dilutions—we can calculate from the conductivities of these salts what the conductivity of a solution containing both of them in equivalent proportions would be. If they are equally dissociated, neither will effect the dissociation of the other, so that we can obtain the conductivity of the mixture by simply taking the average of the conductivities of the components. Now, if sodium sulphide solutions are, as the saponification experiments indicate, merely mixtures of equivalent sodium hydroxide and sodium hydrosulphide solutions, half the molecular conductivity at any dilution should be equal, approximately at least, to the mean of the molecular conductivities of solutions of sodium hydrosulphide and sodium hydroxide at half the dilution. The following table gives the comparison :—

| Dilution. | (Mol. Cond. of $Na_2S$) ÷ 2. | Mean of Mol. Cond. of NaSH and NaOH at (Dilution ÷ 2). |
|---|---|---|
| 32 | 144 | 145 |
| 64 | 149 | 151 |
| 128 | 152 | 156 |

The agreement between the observed and calculated numbers may be deemed sufficient, as the values for sodium hydroxide were interpolated from Kohlrausch's data, which were obtained by the use of a very much purer water than any I could prepare from London water.

All the evidence, then, tends to show that, in dilute aqueous solutions, sodium sulphide is almost entirely hydrolysed into sodium hydroxide and sodium hydrosulphide, while the latter is scarcely attacked by water at all. Thus, when one hydrogen atom of hydrogen sulphide is replaced by an alkali metal, the other hydrogen atom shows scarcely any acid properties, and this is what we might expect from the theory of polybasic acids given by Ostwald.* According to this theory, the nearer the two electrical charges on a bivalent negative ion are to each other, the greater will be the difficulty of forming this ion. Since, now, the two charges in a sulphide would reside on the same atom, it is not surprising that

* *Zeit. physikal. Chem.*, ix. p. 553.

the sulphur ion is formed with difficulty, especially in consideration of the fact that even the first hydrogen atom shows no great tendency to dissociation.

*Sodium Compounds of Acetoacetic Ether, &c.*

We find in chemical literature some diversity of opinion as to whether acetoacetic ether behaves as a stronger or a weaker acid than ethylacetoacetic ether. It seemed to me that this question might be settled by ascertaining the relation between the amounts of hydrolysis in equivalent solutions of the sodium compounds of the two ethers. If the sodium salts are dissolved in water, the solutions are found to be strongly alkaline, but the alkalinity disappears gradually owing to the saponification of the ethers themselves. As, however, the two ethers which are thus saponified are different, a direct comparison of the hydrolysis could not be made in this way, and the sodium salts were consequently dissolved in water containing a large excess of acetic ether, which, according to the law of mass action, would therefore play the predominant part in the saponification. In order that the reaction might not proceed too rapidly, the saponification was conducted at 0°.

The following tables contain the numbers obtained at this temperature with $\frac{1}{50}$-normal solutions of the sodium salts, 5 per cent. of pure ethyl acetate being added in each case. To make the numbers directly comparable, they have been brought to the same calculated original titre by means of a slight correction.

*Sodium Salt of Acetoacetic Ether.*

| Time from beginning. | Titre. |
|---|---|
| 0 | [12·20] |
| 2 | 10·93 |
| 4 | 10·66 |
| 7 | 10·38 |
| 22 | 9·32 |
| 52 | 8·02 |
| 80 | 6·98 |
| 145 | 5·37 |
| 221 | 3·69 |

*Sodium Salt of Ethylacetoacetic Ether.*

| Time from beginning. | Titre. |
|---|---|
| 0 | [12·20] |
| 2 | 6·53 |
| 4·5 | 4·27 |
| 6 | 3·29 |
| 8 | 2·47 |
| 10 | 2·03 |
| 37 | 0·78 |
| 97 | 0·42 |

It appears from these numbers that the saponification proceeds much more rapidly in the case of ethylacetoacetic ether than in the case of acetoacetic ether, so that we must conclude that the former behaves as a much weaker acid than the latter. This conclusion is borne out by determinations of the electric conductivity of their aqueous solutions which have been performed by Walden.* His results for 25° are as follows:—

| | Molec. Conduct. | |
|---|---|---|
| Dilution. | Acetoacetic Ether. | Ethylacetoacetic Ether. |
| 128 | 0·92 | 0·068 |
| 256 | 0·94 | 0·070 |

The same difference between the two ethers thus appears whether we consider the rate of saponification or the electrical conductivity

* *Jour. Russ. Chem. Soc.*, xxiii. p. 639.

**The Pallial Complex of Dolabella.** By **J. D. F. Gilchrist**, M.A., B.Sc., Ph.D. Communicated by **Professor Ewart**, F.R.S. (*From the Natural History Laboratory of the University of Edinburgh.*) (With a Plate.)

(Read May 28, 1894.)

The following account of the pallial complex of the Tectibranch *Dolabella* is, as regards the minuter details, founded on a single specimen (*Dolabella rumphii*) kindly sent me by Professor M'Intosh of St Andrews. By an examination of the collection in the South Kensington Natural History Museum I was enabled to verify for the genus points which might have been peculiar to this species, and in one point to note an important variation.

A few words as to the necessity of an accurate description of the pallial complex or system of organs in the mantle cavity of the *Opisthobranchiata*, and the inferences that may be drawn, may not be out of place.

Just as a large group of the Mollusca, the *Prosobranchiata*, is characterised by the possession of a shell, and the whole organism is moulded and conditioned by this shell, so another group, the *Opisthobranchiata*, is characterised by the getting rid of this shell,—a process which draws in its train the most thorough-going changes in the different organs, especially those of locomotion. The foot becomes largely developed, the shell disappears or becomes enveloped by the mantle, the viscera descend into the foot, and, most important of all, as throwing light upon the problem of the twisting of the Molluscan body, the various organs tend to return to their original position at the hinder end of the body. Deprived of its cumbrous shell there are opened up to the animal modes of life previously unattainable. It may become semi- or wholly pelagic, may crawl about freely among tangled sea-weed, or find its way readily through sand and mud; and these new conditions of life in their turn give rise to new modification of organs. It is this that makes the parapodial and pallial organs of the *Opisthobranchiata* such an interesting and instructive study, and it is in this respect that the specimen under examination is of value.

The animal is 4½ inches long by 2½ broad. It is much wider posteriorly than in front (fig. 1). The general surface of the body, not excepting the rhinophora, is covered with small filamentous protuberances. In many of the other species examined these were much more strongly developed. They were also found in some of the *Notarchidæ*, a group which, as we shall see, resembles the *Dolabellidæ* in other respects. The whole posterior part of the animal presents the appearance of an oblique truncation, round the margin of which these filaments are specially developed.

Anteriorly the head region is well marked, with fore and hind tentacles.

The posterior and broader part consists externally of the highly-developed parapodia (*pa.*). These lateral extensions of the foot have grown upwards on either side and behind, so as to enclose almost completely the visceral region, consisting of viscera, mantle gills, and other pallial organs. The only part of the visceral region visible is the exhalent sipho (*ex.s.*) and part of the mantle (*m.*), which protrude slightly through the small parapodial opening. There is thus formed a large sac-like cavity round the visceral region. As will be seen from fig. 2, a cross section of the animal, somewhat diagrammatic, this cavity extends in under the viscera both at the sides (fig. 2) and behind, (fig. 3),—a condition also characteristic of the *Notarchidæ*, and which may be of importance as indicating a persistence of the prosobranchiate condition where the visceral mass is quite free from the foot.

In *Notarchus* the parapodial opening is even further reduced, being indeed limited to a mere aperture in front, one opening serving both as inhalent and exhalent sipho. Though the parapodial opening is not so far reduced in *Dolabella*, there are special arrangements by which exactly the same physiological condition may be temporarily brought about. Fig. 2 shows that the parapodia (*pa.*) are specially modified at their free borders, so that they can be firmly interlocked; and there is on the posterior end of the visceral mass a cushion or button-like protuberance by which the posterior end of the parapodial opening can be securely closed, there being thus left only the wide and well-marked opening in front. The significance of this arrangement in *Notarchus* is not far to seek, as the animal is often observed, especially when irritated, to inflate this large parapodial bag with water, which is then forcibly ejected, the impetus sending the animal backwards in the opposite direction. It will be remembered that in another group of the Mollusca (the Cephalopods) exactly the same physiological arrangement is brought about by the development, however, of the mantle, not of the parapodia, as here. Probably a similar function is subserved by the arrangement in *Dolabella*, though no direct observations on this point have been recorded.

Mantle: In *Notarchus* the parapodia so completely envelop the visceral mass as to render special protection for the gills superfluous, and hence the mantle has all but disappeared. In this form, however, the parapodial opening is immediately over the gills, extending over their whole length, and we find the mantle well developed and completely covering the gills (fig. 2, *m*). It might be supposed that such an arrangement would be highly unsuitable for the access of water. This is secured in *Notarchus* by the gills being almost entirely uncovered by a mantle, and floating freely in the parapodial cavity. By two special arrangements in *Dolabella*, however, a definite current of water is directed over the gills. First, the front of the mantle is closely attached to the left parapodium, so as to divert the current of water entirely to the right side, on which the gills lie (fig. 3, *a*). Secondly, the posterior rim of the mantle, after being folded up to form an exhalent sipho (fig. 3, *s*), is continued backwards round the anus, and downwards, and then forward over the floor of the mantle cavity to the genital opening (fig. 3, *f* and fig. 2, *f*), thus forming a ridge, the effect of which would be to guide the current of water over the gills and up to the exhalent sipho. This arrangement is of such importance, suggesting as it does a phylogenetic connection between the mantle and the genital furrow, that it requires an accurate examination. Fig. 3, *m*, is intended to show the mantle and its continuation into this fold and ultimately into the genital furrow (*g.f.*). It will be noted that the mantle is not uninterruptedly continuous with this fold, but that there is a small gap of about one mm. in length. This gap (*g.*) may, however, be accounted for by the fact that the button or cushion-like arrangement previously mentioned occurs at this point.

This is proved by the absence of this gap in other *Dolabellæ.* A large *Dolabella* from Samoa in the British Museum showed particularly well the continuation of the fold over this point without any gap. This fold, therefore, must be considered as a continuation of the mantle. The changes that it undergoes in its course along the floor of the pallial cavity till it merges into the genital furrow are shown by fig. 3 A, B, C, D, E, F, which are transverse sections of it at various places. It will be noted that a special secondary fold which covers over the hypobranchial gland is developed on the under side (B, *f'*), the part directed upwards soon disappearing.

Another fact to be noted in the general structure of the mantle is the limited extent to which it is reflected over the shell, thus leaving a large portion of the latter exposed (fig. 2, *sh*). This is exceptional among the *Aplysiidæ*, where the shell is as a rule almost completely covered by the reflected mantle. As in the *Aplysiidæ*, the under side of the mantle is crowded with peculiar large unicellular glands (fig. 4, *gl*, fig. 2, *gl*). A slight fold (fig. 4, *f*, fig. 2, *f*), the significance of which is not very apparent, is also to be noted. It becomes much larger towards the posterior end of the mantle. It does not appear in *Aplysia.*

The shell is peculiar in shape and calcification, but presents no specially important features. As a faithful record of the course of development of the mantle it reflects the peculiar shape of that organ, and especially that part of it modified into an exhalent organ, the sipho.

The osphradium presents characteristics which again bring the animal nearer *Notarchus.* I have elsewhere figured and described the peculiar lid or fold of skin in *Notarchus*, whereby the osphradium can be completely covered up, and suggested that this may be connected with the fact that not only the pure incoming stream of water, but also the impure outgoing stream, pass over this sensory organ, which by this fold may be protected from unnecessary irritation. An arrangement much similar to this, though differing in details, occurs in the form under examination and may perhaps be satisfactorily explained in the same way. Fig. 5, a section through the mantle (*m.*) and efferent vessel, on the under side of which the osphradial pit occurs, shows this. The lip or fold of skin (fig. 5, *os.l.*) is well marked. The peculiar state of the osphradial ganglion is also to be seen in the same fig. (*os.g.*). In *Aplysia* the ganglion cells are concentrated into a definite osphradial ganglion and into a definite branchial ganglion some distance along the branchial nerve. Here, however, the cells are scattered almost indiscriminately along the nerve. This condition throws some light upon the question as to the homology of the various ganglia, occurring at this point. The position of the osphradium on the efferent vessel differs from that in *Notarchus*, where it is more in front, and resembles that of *Aplysia*, where it is decidedly under the vessel. In connection with the innervation of the osphradium it may be stated that the visceral ganglion (the right), from which the osphradial nerve comes, was found to be lying over on the left, and consequently the pleuro-visceral connectives were crossed,—that is, the arrangement was chiastoneure.

Nothing calling for special attention is to be noted in the gills.

The position of the anus in all these forms is important, as throwing light upon the difficult problem of the torsion of the Molluscan body. In a general account of the pallial organs of the *Opisthobranchiata* I have endeavoured to show that there is in co-ordination with the loss of the shell a tendency of the different organs to return to their original position at the posterior end of the body, and that, as a rule, the anus is the leading organ in this backward movement. In *Aplysia* the anus *practically* reaches the posterior median position, by the aid of an exhalent sipho, in the middle of which it opens. In the form under consideration, however, this sipho (fig. 3, *s*) has been moved forward towards the head region, and is to a large extent unavailable for this purpose. We find, accordingly, that here the anal opening has shifted further to the posterior median position, to such a degree that it has all but broken through the edge of the mantle and passed over it (fig. 3, *a.n.*). This condition explains how the arrangement in *Notarchus* may have been phylogenetically brought about. With the gradual growth of the parapodia behind, the sipho has been shifted further and further forward, followed by the anus; while the part of the mantle behind the sipho has gradually disappeared, so that finally, as in *Notarchus*, only a part in front is left, and that for a special function connected with conjugation, as explained elsewhere.

The position of the hypobranchial gland is peculiar. It occurs, not as in other forms, on the floor of the mantle cavity, but lies in the hollow formed by the junction of it with the parapodium of the right side, the cellular elements of the gland being indeed partly imbedded in the parapodial tissue, as shown in fig. 3, C.*h.g.* The unicellular elements of this gland open to the exterior, not by a common duct, but each glandular cell by a duct of its own.

The position of the genital opening (fig. 3, *g.o.*), which of all the pallial organs shows the least tendency to shift its position, is very similar to that in *Aplysia* and *Notarchus.* At the genital opening the furrow noted before as a continuation of the mantle undergoes some slight modification, as shown by section E, but it nevertheless is distinctly continuous with and merges into the upper and larger lip of the genital furrow.

### *General Conclusions.*

The pallial complex of *Dolabella* is an interesting connecting link and perhaps transition stage between that of *Aplysia* and that of *Notarchus.* The general arrangement in the parapodial cavity and the condition of the osphradium indicate a functional activity similar to that in *Notarchus*, and bring the animal nearer the latter than *Aplysia.* The position of the anus also is of special importance in this respect.

But *Dolabella* differs both from *Aplysia* and *Notarchus* in the possession of the remarkable fold connecting mantle and genital furrow. The significance of this fold is not very apparent. It seems at first sight to suggest a homology between the mantle and the upper lip of the genital groove. We could well fancy that the mantle edge functioned as a sperm conductor at a time when the mantle cavity was further forward than it now is; that on the retreat of the mantle cavity, while the genital opening remained stationary, the mantle retained its organic connection with the sperm groove by means of this fold. Against this supposition, on the other hand, are the facts that both the genital duct and the hypobranchial gland are outside this fold; that is outside the mantle cavity,—an unlikely condition. Perhaps the point had better be left for decision till something is known of the development of the various parts.

Other points of importance to be considered, however, in a review of the whole group are the undifferentiated condition of the ctenidial and osphradial ganglia and the chiastoneure condition of the pleuro-visceral connectives.

I have, in conclusion, to express my obligation to Professor M'Intosh and the authorities of the South Kensington Museum for the material for this investigation; and to Professor Ewart, who afforded me every facility for carrying out the work in the Zoological Laboratory of the University of Edinburgh.

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## EXPLANATION OF PLATE.

Fig. 1. *Dolabella rumphii*, *pa*, parapodia ; *m*, mantle ; *s*, exhalent sipho.

Fig. 2. Diagrammatic cross section of *Dolabella rumphii*, *pa*, parapodia ; *sh*, shell ; *m*, mantle ; *f'*, fold ; *gl*, mantle gland ; *ct*, gill ; *f*, fold ; *h.g.*, hypobranchial gland.

Fig. 3. *Dolabella rumphii*, right parapodium removed, *a*, attachment of mantle to left parapodium ; *an*, anus ; *f*, fold ; *g*, gap in fold ; *g.f.*, genital furrow ; *g.o.*, genital opening ; *h.g.*, hypobranchial gland ; *m*, mantle ; *pa*, left parapodium.

Fig. 4. Transverse section of mantle, *sh*, shell ; *f*, fold ; *gl*, mantle glands.

Fig. 5. Section through mantle and osphradium, *c*, glandular protuberance of mantle ; *ct.n.*, ctenidial nerve ; *m*, mantle ; *os.g.*, osphradial ganglion ; *s.l.*, lid closing osphradial pit ; *os.n.*, nerve to osphradium ; *sh*, shell.

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DR. GILCHRIST ON THE PALLIAL COMPLEX OF DOLABELLA.

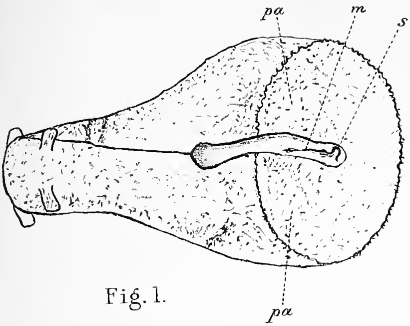



Fig. 1.

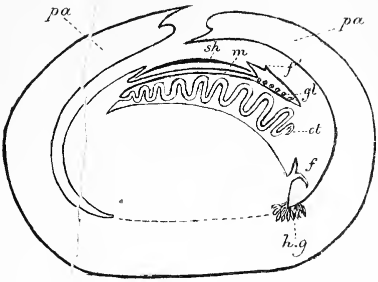



Fig. 2.

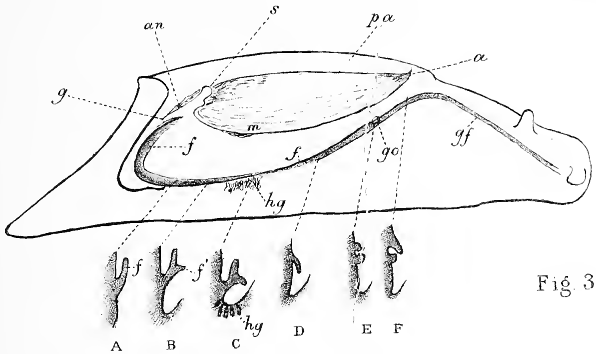



Fig. 3.

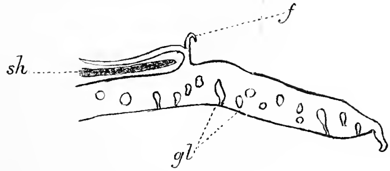



Fig. 4.

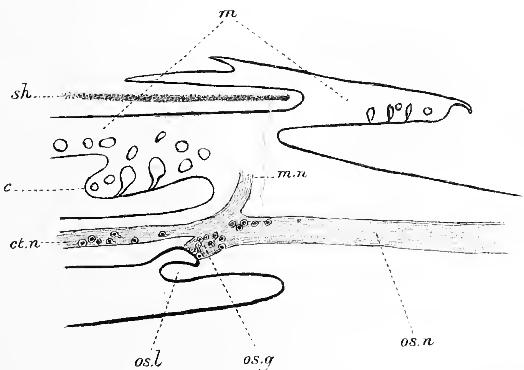



Fig. 5.

McFarlane & Erskine, Lithrs Edinr

## Coordinates versus Quaternions. By Professor Cayley.

(Read July 2, 1894.)

It is contended that Quaternions (as a method) are more comprehensive and less artificial than—and, in fact, in every way far superior to—Coordinates. Thus Professor Tait, in the Preface to his *Elementary Treatise of Quaternions* (1867), reproduced in the second and third editions (1873 and 1890), writes—"It must always be remembered that Cartesian methods are mere particular cases of quaternions where most of the distinctive features have disappeared; and that when, in the treatment of any particular question, scalars have to be adopted, the quaternion solution becomes identical with the Cartesian one. Nothing, therefore, is ever lost, though much is generally gained, by employing quaternions in place of ordinary methods. In fact, even when quaternions degrade to scalars, they give the solution of the most general statement of the problem they are applied to, quite independent of any limitations as to choice of particular coordinate axes." And he goes on to speak of "such elegant trifles as trilinear coordinates," and would, I presume, think as lightly of quadriplanar coordinates. It is right to notice that the claims of quaternions are chiefly insisted upon in regard to their applications to the physical sciences; and I would here refer to his paper, "On the Importance of Quaternions in Physics" (*Phil. Mag.*, Jan. 1890), being an abstract of an address to the Physical Society of the University of Edinburgh, Nov. 1889; but these claims certainly extend to and include the science of geometry.

I wish to examine into these claims on behalf of quaternions. My own view is that quaternions are merely a particular method, or say a theory, in coordinates. I have the highest admiration for the notion of a quaternion; but (I am not sure whether I did or did not use the illustration many years ago in conversation with Professor Tait), as I consider the full moon far more beautiful than any moonlit view, so I regard the notion of a quaternion as far more beautiful than any of its applications. As another illustration which I gave him, I compare a quaternion formula to a pocket-map—a capital thing to put in one's pocket, but which for use must be unfolded: the formula, to be understood, must be translated into coordinates.

I remark that the imaginary of ordinary algebra—for distinction call this $\theta$—has no relation whatever to the quaternion symbols $i, j, k$; in fact, in the general point of view, all the quantities which present themselves are, or may be, complex values $a+\theta b$, or, in other words, say that a scalar quantity is in general of the form $a+\theta b$. Thus quaternions do not properly present themselves in plane or two-dimensional geometry at all—although, as will presently appear, we may use them in plane geometry; but they belong essentially to solid or three-dimensional geometry, and they are most naturally applicable to the class of problems which in coordinates are dealt with by means of the three rectangular coordinates $x$, $y$, $z$.

In plane geometry, considering an origin O, and through it two rectangular axes O$x$, O$y$, then in coordinates we determine the position of a point by means of its coordinates $x$, $y$; or, writing $x$, $y$, $z$ to denote given linear functions of the original rectangular coordinates $x$, $y$, we may, if we please, determine it by trilinear coördinates, or say by the ratios $x : y : z$. The advantage is, that we thereby deal with the line infinity as with any other line, whereas with the rectangular coordinates $x$, $y$ the line infinity presents itself as a line *sui generis*, and that we thereby bring the theory into connection with that of the homogeneous functions $(*\between x, y, z)^n$.

In quaternions, the position of a point is determined in reference to the fixed point O, by its vector $\alpha$, which is in fact $=ix+jy$, where $i, j$ are the quaternion imaginaries ($i^2=-1$, $j^2=-1$, $ij=-ji$), but the idea is to use as little as possible the foregoing equation $\alpha=ix+jy$, and thus to conduct the investigations independently, as far as may be, of the particular positions of the axes O$x$, O$y$.

As the most simple example, take the theorem that the lines joining the extremities of equal and parallel lines in a plane are themselves equal and parallel, viz. (writing   to denote equal and parallel), if AB ∾ CD, then AC ∾ BD.

*Coordinates.*

A, B, C, D are determined by their coordinates

$(x_1, y_1)$, $(x_2, y_2)$, $(x_3, y_3)$, $(x_4, y_4)$.

AB ∾ CD gives

$$\left.\begin{array}{l} x_2 - x_1 = x_4 - x_3, \\ y_2 - y_1 = y_4 - y_3, \end{array}\right\}, \text{ whence}$$

$$\left.\begin{array}{l} x_3 - x_1 = x_4 - x_2 \\ y_3 - y_1 = y_4 - y_2, \end{array}\right\}, \text{ that is}$$

AC ∾ BD.

*Quaternions.*

AB, CD are determined by their vectors $\alpha$, $\beta$, and then writing $\gamma$ for the vector AD.

AB ∾ CD gives $\alpha = \beta$,

whence

$$\gamma - \beta = -\alpha + \gamma,$$

that is

AC ∾ BD.

And for the comparison of the two solutions we have

$$\alpha = i(x_2 - x_1) + j(y_2 - y_1), \quad \beta = i(x_4 - x_3) + j(y_4 - y_3).$$

But this example of a plane theorem is a trivial one, given only for the sake of completeness.

Passing to solid geometry, we have—

*Coordinates.*—Considering a fixed point O, and through it the rectangular axes O$x$, O$y$, O$z$, the position of a point is determined by its coordinates $x$, $y$, $z$. But we may, in place of these, consider the quadriplanar coordinates $(x, y, z, w)$ linear functions of the original rectangular coordinates $x$, $y$, $z$.

*Quaternions.*—The position of a point in reference to the fixed origin O is determined by its vector $\alpha$, which is in fact $= ix + jy + kz$ (where $i, j, k$ are the Hamiltonian symbols $(i^2 = j^2 = k^2 = -1$, $jk = -kj = i$, $ki = -ik = j$, $i = -ji = k))$, but the idea is to use as little as possible the foregoing equation $\alpha = ix + jy + kz$, and thus to conduct the investigations independently, as far as may be, of the particular positions of the axes O$x$, O$y$, O$z$.

I consider the problem to determine the line OC at right angles to the plane of the lines OA, OB.

*Coordinates.*

Taking O as origin, the coordinates of A, B, C are taken to be $(x_1, y_1, z_1)$, $(x_2, y_2, z_2)$, $(x, y, z)$ respectively. Then

$$xx_1 + yy_1 + zz_1 = 0\,,$$
$$xx_2 + yy_2 + zz_2 = 0\,;$$

whence

$$x : y : z = y_1z_2 - y_2z_1 : z_1x_2 - z_2x_1 : x_1y_2 - x_2y_1\,.$$

*Quaternions.*

Points A, B, C are determined by their vectors $\alpha$, $\beta$, $\gamma$. Then

$$S\alpha\gamma = 0, \quad S\beta\gamma = 0\,;$$

whence

$$m\gamma = V\alpha\beta\,,$$

$m$ being an arbitrary scalar.

Here to compare the two solutions, observe that the two equations $S\alpha\gamma = 0$, $S\beta\gamma = 0$ are in fact the equations $xx_1 + yy_1 + zz_1 = 0$, $xx_2 + yy_2 + zz_2 = 0$; and so also $m\gamma = V\alpha\beta$ denotes the relations $x : y : z = y_1z_2 - y_2z_1 : z_1x_2 - z_2x_1 : x_1y_2 - x_2y_1$. But a quaternionist says that $m\gamma = V\alpha\beta$ *is* the compendious and elegant solution of the problem as opposed to the artificial and clumsy one $x : y : z = y_1z_2 - y_2z_1 : z_1x_2 - z_2x_1 : x_1y_2 - x_2y_1$. And it is upon this that I join issue; $m\gamma = V\alpha\beta$ is a very pretty formula, like the folded-up pocket-map, but, to be intelligible, I consider that it requires to be developed into the other form. Of course, the example is as simple a one as could have been selected; and, in the case of a more complicated example, the mere abbreviation of the quaternion formula would be very much greater, but just for this reason there is the more occasion for the developed coordinate formula. To take another example, the condition, in order that the vectors $\alpha$, $\beta$, $\gamma$ may be coplanar, is $S\alpha\beta\gamma = 0$, and Professor Tait contrasts this with the prolixity of the corresponding coordinate formula

$$\begin{vmatrix} x, & y, & z \\ x_1, & y_1, & z_1 \\ x_2, & y_2, & z_2 \end{vmatrix} = 0\,.$$

I remark that, when all the components of a determinant have to be expressed, nothing can be shorter than this, the ordinary determinant notation, which simply expresses the several components in their line-and-column relation to each other. But as a mere abbreviation, it would be allowable to write $\Delta, =(ABC)$ to denote the determinant formed by the coordinates of the three points.

In conclusion, I would say that while coordinates are applicable to the whole science of geometry, and are the natural and appropriate basis and method in the science, quaternions seem to me a particular and very artificial method for treating such parts of the science of three-dimensional geometry as are most naturally discussed by means of the rectangular coordinates $x$, $y$, $z$.

## On the Intrinsic Nature of the Quaternion Method.

By **Prof. Tait.**

(Read July 2, 1894.)

My title is purposely ambiguous, because it has to represent two things—I intend to treat not only of what a quaternion really is, but also of its self-containedness, or independence.

Professor Cayley has just stated that "while co-ordinates are applicable to the whole science of geometry, and are the natural and appropriate basis and method in the science, Quaternions seem to me a particular and very artificial method for treating such parts of the science of three-dimensional Geometry as are most naturally discussed by means of the rectangular co-ordinates $x$, $y$, $z$."

On this I would remark as follows :—

1. I have always maintained that it is not only not a reproach to, but one of the most valuable characteristics of, Quaternions that they are uniquely adapted to tridimensional space. In my Address to Section A, at the British Association Meeting in 1871, I said :—

"It is true that, in the eyes of the pure mathematician, Quaternions have one grand and fatal defect. They cannot be applied to space of $n$ dimensions, they are contented to deal with those poor three dimensions in which mere mortals are doomed to dwell, but which cannot bound the limitless aspirations of a Cayley or a Sylvester. From the physical point of view this, instead of a defect, is to be regarded as the greatest possible recommendation. It shows, in fact, Quaternions to be a special instrument so constructed for application to the *Actual* as to have thrown overboard everything which is not absolutely necessary, without the slightest consideration whether or no it was thereby being rendered useless for applications to the *Inconceivable.*"

2. Whether Quaternions are to be regarded as artificial, or the reverse, will obviously depend wholly upon what is to be understood by the term Quaternions. This forms the main object of the present paper.

3. Though the passage quoted above contains no statement as to the relative merits of Quaternions, and Co-ordinates, as instruments (in the region which is common to them) it is clear from other passages in his paper that Prof. Cayley holds that Quaternions are, at best, superfluous :—he allows that they enable us to effect great abbreviations, but he insists that, to be applied or even understood, they must be reconverted into the $x$, $y$, $z$ elements of which they are, in his view, necessarily composed.

But another great analyst, who certainly devoted vastly more time and attention to Quaternions than it can have been possible for Prof. Cayley to devote, took a very different view of the matter :—

"It is particularly noteworthy that [Quaternions were] invented by one of the most brilliant Analysts the world has yet seen, a man who had for years revelled in floods of symbols rivalling the most formidable combinations of Lagrange, Abel, or Jacobi. For him the most complex trains of formulæ, of the most artificial kind, had no secrets :—he was one of the very few who could afford to dispense with simplifications : yet, when he had tried quaternions, he threw over all other methods in their favour, devoting almost exclusively to their development the last twenty years of an exceedingly active life."

It will be gathered from what precedes that, in my opinion, the term Quaternions means one thing to Prof. Cayley and quite another thing to myself :—thus

To Prof. Cayley Quaternions are mainly a Calculus, a species of Analytical Geometry; and, as such, *essentially* made up of those co-ordinates which he regards as "the natural and appropriate basis of the science." They artfully conceal their humble origin, by an admirable species of packing or folding :—but, to be of any use, they

——— doubly dying, must go down
To the vile dust from which they sprung!

To me Quaternions are primarily a Mode of Representation :—immensely superior to, but of essentially the same kind of usefulness as, a diagram or a model. They *are*, virtually, the thing represented: and are thus antecedent to, and independent of, coordinates : giving, in general, all the main relations, in the problem to which they are applied, without the necessity of appealing to coordinates *at all.* Co-ordinates may, however, easily be *read into* them :—when anything (such as metrical or numerical detail) is to be gained thereby. Quaternions, in a word, *exist* in space, and we have only to recognize them :—but we have to *invent* or *imagine* coordinates of all kinds. The grandest characteristic of Quaternions is their transparent intelligibility. They give the spirit, as it were, leaving the mere letter aside, until or unless, it seems necessary to attend to that also. In this respect they give a representation analogous to the real image of a planet in the focus of an object-glass or mirror: —all that is obtainable is *there*, and you may apply your microscopes and micrometers to it if you please. But, theoretically at least, you may dispense with them and have recourse to your eyes and your yard-stick alone, if you increase the focal length, and along with it the aperture, of your object-glass sufficiently. Of course Newton's "most serene and quiet air" would be indispensable. For the development of this feature of my subject, and for illustrative examples, I refer to the B. A. Address above cited; and to the Address to the Edinburgh University Physical Society, alluded to by Prof. Cayley.

To those who have read Poe's celebrated tale, *The Purloined Letter*, it will be obvious that the contrast between these two views of Quaternions is even greater than that between the Parisian Police and M. Dupin himself, though of very much the same kind.

There was a time, in their early history, when Professor Cayley's view of Quaternions was not merely a correct one, it was the *only* possible one. But, though the name has not been altered, the thing signified has undergone a vital change. To such an extent, in fact, that we may almost look upon the quaternion of the latter half of this century as having, from at least one point of view, but little relation to that of the seven last years of the earlier half.

Hamilton's extraordinary *Preface* to his first great book shows how from Double Algebras, through Triplets, Triads, and Sets, he finally reached Quaternions. This was the genesis of the Quaternion of the forties, and the creature then produced is still essentially the Quaternion of Prof. Cayley. It is a magnificent analytical conception; but it is nothing more than the full development of the system of imaginaries $i, j, k$; defined by the equations

$$i^2 = j^2 = k^2 = ijk = -1,$$

with the associative, but *not* the commutative, law for the factors. The novel and splendid points in it were the treatment of all directions in space as essentially alike in character, and the recognition of the unit vector's claim to rank also as a quadrantal versor. These were indeed inventions of the first magnitude, and of vast importance. And here I thoroughly agree with Prof. Cayley in his admiration. Considered as an analytical system, based throughout on pure imaginaries, the Quaternion method is elegant in the extreme. But, unless it had been *also* something more, something very different and much higher in the scale of development, I should have been content to admire it:—and to pass it by.

It has always appeared to me that, magnificent as are Hamilton's many contributions to mathematical science:—his Fluctuating Functions, and his Varying Action, for instance:—nothing that he (or indeed any other man) ever did in such matters can be regarded as a higher step in pure reasoning than that which he took when he raised Quaternions from the comparatively low estate of a mere system of *Imaginaries* to the proud position of an Organ of Expression; giving simple, comprehensive, and (above all) transparently intelligible, embodiment to the most complicated of *Real* geometrical and physical relations. *From the most intensely artificial of systems arose, as if by magic, an absolutely natural one!*

Most unfortunately, alike for himself and for his grand conception, Hamilton's nerve failed him in the composition of his first great Volume. Had he then renounced, for ever, all dealings with $i, j, k$, his triumph would have been complete. He spared Agag, and the best of the sheep, and did not utterly destroy them! He had a paternal fondness for $i, j, k$; perhaps also a (not unnatural) liking for a meretricious title such as the mysterious word *Quaternion;* and, above all, he had an earnest desire to make the utmost return in his power for the liberality shown him by the authorities of Trinity College, Dublin. He had fully recognized, and proved to others, that his $i, j, k$ were mere excrescences and blots on his improved method:—but he unfortunately considered that their continued (if only partial) recognition was indispensable to the reception of his method by a world steeped in Cartesianism! Through the whole compass of each of his tremendous volumes one can find traces of his desire to avoid even an allusion to $i, j, k$; and, along with them, his sorrowful conviction that, should he do so, he would be left without a single reader. There can be little doubt that, by thus taking a course which he *felt* to be far beneath the ideal which he had attained, he secured for Quaternions at least the temporary attention of mathematicians. But there seems to me to be just as little doubt that in so doing he led the vast majority of them to take what is still Professor Cayley's point of view; and thus, to regard Quaternions as (apparently at least) obnoxious to his criticisms. And I further believe that, *to this cause alone*, Quaternions owe the scant favour with which they have hitherto been regarded.

[I am quite aware that, in making such statements, I inferentially condemn (to some extent, at least) the course followed in my own book. But, since my relations with Hamilton in the matter have been alluded to more than once, and alike incompletely and incorrectly, by Hamilton's biographer, I may take this opportunity of making a slight explanation, not perhaps altogether uncalled for. That Hamilton can altogether have forgotten the permission (limited as it was) which he had given me, when, a little later, I proposed to avail myself of it (*strictly within the limits imposed*) seems incredible. Mr Graves should either have let the matter alone, or have gone into much greater detail about it. As it stands, he virtually represents Hamilton as being unaccountably capricious. The following extract from the letter (of date July 10, 1859) in which Hamilton gave his sanction to my writing a book on the subject, speaks for itself. I had, of course, no rights in the matter :—and I cheerfully submitted to the restrictions he imposed on me; especially as I understood that he expressly (and most justly) desired to be the first to give to the world his system in its vastly improved form.

"[2] If I shall go on to speak of my views, wishes, or feelings, on the subject of future publication, I request you beforehand to give to any such expression of mine your most indulgent construction; and not to attribute to me any jealousy of you, or any wish to interfere, in any way, with your freedom, as Author and as Critic.

[3.] If we were altogether strangers, I could have no right to address you on such a subject at all. [Here follow, as an example, some allusions (which need not be quoted) to a then recent pamphlet of Möbius, dealing with the Associative Principle in Quaternion Multiplication.] But between you and me, the case is perhaps not exactly similar; as we have so freely corresponded, and as you are an Author in the same language, and of the same country :—England, Scotland, and Ireland, being here held to have their sons compatriots.

[4.] To Möbius's excellent Pamphlet, it is likely that I may return. Meanwhile I trust that it cannot be offensive to you, if I confess,—what indeed your No. 38 encourages me to state,—that in any such future publication on the Quaternions as you do me the honour to meditate, I should *prefer* the *establishment* of 'PRINCIPLES' being left, for some time longer,—say even 2 or 3 years,—in my own hands. Open to improvement as my treatment of them confessedly is, I wish that improvement, at least to some extent, to be made and published by myself. Briefly, I should like (I own it) that no book, so much more attractive to the mathematical public than any work of mine, as a book of yours is likely to be, should have the appearance of laying a 'FOUNDATION': although the richer the 'SUPERSTRUCTURE,' on a previously laid foundation, may be, the better shall I be pleased. I think, therefore, that you may be content to *deduce* the Associative Law, from the *rules* of $i, j, k$; leaving it to me to consider and to *discuss* whether it might not have been a fatal *objection* to these *rules*, if they had been found to be *inconsistent* with that PRINCIPLE.

. . . . . . . . .

[7.] For calculation, you know, the *rules* of $i, j, k$ are a *sufficient basis*, although of course we have continual need for transformations, such as

$$V\gamma V\beta\alpha = \alpha S\beta\gamma - \beta S\gamma\alpha,$$

which may at last be reduced to *consequences* of those rules; and also require some *Notation*, such as S,V,K,T,U, which I have been glad to find that you are willing, *at least for the present*, to retain and to employ. But my peculiar turn of mind makes me dissatisfied without seeking to go deeper into the *philosophy* of the whole subject, although I am conscious that it will be imprudent to attempt to gain any lengthened hearing for my reflections. In fact I hope to get much more rapidly on to *rules* and *operations*, in the MANUAL than in the LECTURES; although I cannot consent to neglect the occasion of developing more fully my *conception* of the MULTIPLICATION OF VECTORS, and of seeking to establish such mult[iplication] as a much *less arbitrary* process, than it may seem to most readers of my former book to be."

I do not now think that Hamilton, with the "peculiar turn of mind" of which he speaks, could ever, *in a book*, have conveyed adequately to the world his new conception of the Quaternion. I got it from him by correspondence, and in conversation. When he was pressed to answer a definite question, *and could be kept to it*, he replied in ready and effective terms, and no man could express *vivâ voce* his opinions on such subjects more clearly and concisely than he could :—but he perpetually planed and repolished his printed work at the risk of attenuating the substance : and he fatigued and often irritated his readers by constant excursions into metaphysics. One of his many letters to me gave, in a few dazzling lines, the whole substance of what afterwards became a Chapter of the *Elements ;* and some of his *shorter* papers in the *Proc. R. I. A.* are veritable gems. But these were dashed off at a sitting, and were not planed and repolished.

Should I be called upon, in the future, to produce a fourth edition of my book, the Chapter which Prof. Cayley so kindly furnished for the third edition will probably preserve by far the greater part of the allusions to $i$, $j$, $k$ (except, of course, the necessary introductory and historical ones) which it will contain.]

In the sense above explained, I consider Prof. Cayley's remarks to be so far warranted, hard to bear though some of them undoubtedly are. But the Quaternion, when it is regarded from the true point of view, is seen to be untouched, in fact unassailable, by any criticism based upon such grounds as reference to co-ordinates. It occupies a region altogether apart. To compare it to a pocket map is to regard it as a mere artificial mode of wrapping up and concealing the $i$, $j$, $k$ or the $x$, $y$, $z$ which are supposed to be its ultimate constituents. To be of any use it must be unfolded, and its neatly hidden contents turned out. But, from my point of view, this comparison is entirely misleading. The quaternion exists, as a space-reality, *altogether independent of* and *antecedent to* $i$, $j$, $k$ or $x$, $y$, $z$. *It* is the natural, *they* the altogether artificial, weapon. And I venture further to assert (1) that if Descartes, or some of his brilliant contemporaries, had recognized the quaternion, (and it is quite conceivable that they might have done so), science would have then advanced with even more tremendous strides than those which it has recently taken ; and (2) that the wretch who, under such conditions, had ventured to introduce $i$, $j$, $k$, would have been justly regarded as a miscreant of the very basest and most depraved character; possibly subjected to "brave punishments," the *peine forte et dure* at the very least! In a word, Hamilton INVENTED the Quaternion as Prof. Cayley sees it; he afterwards DISCOVERED the Quaternion as I see it.

If Quaternions are to be compared to a map, at all, they ought to be compared to a *contoured* map or to a model in relief, which gives not only all the information which can be derived from the ordinary map but something more:—something of the very highest importance as regards the features of a country.

A much more natural and adequate comparison would, it seems to me, liken Co-ordinate Geometry (Quadriplanar or ordinary Cartesian) to a steam-hammer, which an expert may employ on any destructive or constructive work *of one general kind*, say the cracking of an egg-shell, or the welding of an anchor. But you must have your expert to manage it, for without him it is useless. He has to toil amid the heat, smoke, grime, grease, and perpetual din of the suffocating engine-room. The work has to be brought to the hammer, for it cannot usually be taken to its work. And it is not in general, transferable; for each expert, as a rule, knows, fully and confidently, the working details of his own weapon only. Quaternions, on the other hand, are like the elephant's trunk, ready at *any* moment for *anything*, be it to pick up a crumb or a field-gun, to strangle a tiger, or to uproot a tree. Portable in the extreme, applicable anywhere:—alike in the trackless jungle and in the barrack square:—directed by a little native who requires no special skill or training, and who can be transferred from one elephant to another without much hesitation. Surely this, which adapts itself to its work, is the grander instrument! But then, *it* is the natural, the other the artificial, one.

The naturalness of Quaternions is amply proved by what they have effected on their first application to well-known, long threshed-out, plane problems, such as seemed particularly ill-adapted to treatment by an essentially space-method. Yet they gave, at a glance, the kinematical solution (perfectly obvious, no doubt, *when found*) of that problem of Fermat's which so terribly worried Viviani! And, without them, where would have been even the Circular Hodograph, with its marvellous power of simplifying the elementary treatment of a planet's orbit? I could give many equally striking instances.

As to the necessity, in modern mathematical physics, for *some* substitute for what I must (with all deference to Prof. Cayley) call the cumbersome, unnatural, and unwieldy mechanism of co-ordinates, I have elsewhere fully expressed my own opinion, and need not repeat it.

Of course it will be obvious from what precedes that I adhere to every word of the first extract which Professor Cayley has made from my original *Preface.*

The phrase which he afterwards extracts for comment :—"such elegant trifles as Trilinear Co-ordinates" :—seems somewhat too sweeping, and I should certainly hesitate to use it without qualification. But the context shows that, in my *Preface*, it was used to characterize the so-called "Abridged Notation" which had then been for some years introduced into Cambridge reading and examinations, not at all because of its superiority in completeness to the ordinary $x$, $y$ system :—and therefore not on scientific grounds :—but mainly for the purpose of "aggravating" students, whether in the lecture-room or in the Senate House, at very small additional labour on the part of the lecturer or the examiner. But I made no reference whatever to Quadriplanar Co-ordinates ; for which I feel all due respect, not altogether free from an admixture of wholesome awe !

## On the Application of Van der Waals' Equation to the Compression of Ordinary Liquids. By Prof. Tait.

(Read June 4, 1894.)

In a paper, read for me to the Society in January last (*ante*, p. 245), I pointed out the difficulties I had met with in trying to reconcile Van der Waals' equation with Amagat's experimental data for common liquids, and I promised to recur to the question when the state of my health should permit. I now find that, as I had then only surmised, the constants in Van der Waals' equation necessarily become non-real when we try to adjust it to Amagat's data.

The proof of this assertion is very simple. Suppose the equation

$$\left(p+\frac{A}{v^2}\right)(v-\beta)=BT$$

to hold for any three pairs of values of $p$ and $v$; say $p$ and $a$, $q$ and $b$, $r$ and $c$. Eliminating BT among the three resulting equations, we have

$$\beta=\frac{\left(pa+\frac{A}{a}\right)-\left(qb+\frac{A}{b}\right)}{\left(p+\frac{A}{a^2}\right)-\left(q+\frac{A}{b^2}\right)}=\frac{\left(qb+\frac{A}{b}\right)-\left(rc+\frac{A}{c}\right)}{\left(q+\frac{A}{b^2}\right)-\left(r+\frac{A}{c^2}\right)}.$$

The values of A are therefore to be found from the quadratic

$$A^2\frac{(a-b)(b-c)(c-a)}{a^2b^2c^2}+A\Sigma\left(p\frac{b-c}{b^2c^2}(ab-bc+ca)\right)-\Sigma(pq(a-b))=0.$$

Write, for brevity,

$$P=p\frac{b-c}{b^2c^2},\quad Q=q\frac{c-a}{c^2a^2},\quad R=r\frac{a-b}{a^2b^2};$$

so that one at least of P, Q, R is essentially negative, if $p$, $q$, $r$ be all positive. The condition that the values of A shall be real is

$$\{\Sigma(P(ab-bc+ca))\}^2+4\Sigma(PQ(a-b)^2c^2)>0.$$

But it is an obvious theorem of ordinary algebra, that, whatever be the quantities involved, the two expressions

$$(lx+my+nz)^2+(xy(l-m)^2+yz(m-n)^2+zx(n-l)^2)$$

and

$$(x+y+z)(l^2x+m^2y+n^2z)$$

are absolutely identical except in form.

Hence the condition for real values of A is simply that

$$\left(P+Q+R\right)\left(P(ab-bc+ca)^2+Q(ab+bc-ca)^2+R(-ab+bc+ca)^2\right)$$

shall be positive:—*i.e.* that its factors shall have the same sign.

To compare with experiment, let us take $r=1$ atm., $c=1$; and find the relation between the values of $p$ and $q$, the pressures when the volume is reduced to $a=0{\cdot}9$, and $b=0{\cdot}95$, respectively.

The factors of the above quantity are

$$-p\frac{0{\cdot}05}{(0{\cdot}95)^2}+q\frac{0{\cdot}1}{(0{\cdot}9)^2}-\frac{0{\cdot}05}{(0{\cdot}95)^2(0{\cdot}9)^2}$$

and

$$-p\frac{0{\cdot}05(0{\cdot}805)^2}{(0{\cdot}95)^2}+q\frac{0{\cdot}1(0{\cdot}905)^2}{(0{\cdot}9)^2}-\frac{0{\cdot}05(0{\cdot}995)^2}{(0{\cdot}95)^2(0{\cdot}9)^2}$$

or, quite approximately enough for our purpose,

$$-p+2{\cdot}228q-1{\cdot}234$$

and

$$-p+2{\cdot}816q-1{\cdot}886\,.$$

In the latter form each has been divided by the (essentially positive) multiplier of $p$; and, as $p$ and $q$ are each of the order 1000 atm., the last terms may usually be disregarded. Thus it appears that the values of A cannot be real if $p/q$ lie between the approximate limits 2·23 and 2·82. But from Amagat's data we easily calculate the following sufficiently accurate values:—

*Ratio of Pressures at* 0° *C. for Volumes* 0·9 *and* 0·95.

| Water. | Bisulphide of Carbon. | Methylic Alcohol. | Ethylic Alcohol. | Chloride of Ethyl. | Propylic Alcohol. | Ether. |
|---|---|---|---|---|---|---|
| 2·51 | 2·61 | 2·65 | 2·65 | 2·69 | 2·71 | 2·73 |

[The values of $q$ range from 458 atm. in the case of ether to 1166 atm. in that of water.] All of these ratios lie well within the limits of the region in which the constants of Van der Waals' equation are non-real; though they are, as a rule, nearer to the upper than to the lower limit.

But it is well to inquire what values A assumes at the limits of this region, when it has just become real. A rough calculation shows that when $p/q = 2{\cdot}23$ we have $A = -18{\cdot}1q$ (a *tension*); and for $p/q = 2{\cdot}82$, $A = 20q$. Outside these limits A has of course two values.

It thus appears that Van der Waals' equation becomes altogether meaningless except for liquids in which the compressibility alters very much with increase of pressure:—*i.e.* for substances which have *just* assumed the liquid form under considerable pressure. For, of course, under the lower limit we are dealing with substances naturally in a state of tension. As I said in my previous paper, this state of things is due mainly to the factor $1/v^2$ with which A (if taken as corresponding to my Π) is affected. There is little doubt that the Π term in my formula does increase as the volume is diminished, but much more slowly than in the inverse ratio of the square of the volume.

(Added, 6/6/94.) It may be interesting to look at the above result from a different point of view, so as to find *why* it is impossible to reconcile the general equation of Van der Waals with the experiments of Amagat.

For this purpose let us take $\beta$ as independent variable, and (using the same data as before) find the value of $p/q$. Eliminating BT and A, we obtain the equation

$$\Sigma\left(p\frac{b^2-c^2}{b^2c^2}(a-\beta)\left(\frac{bc}{b+c}-\beta\right)\right)=0\,;$$

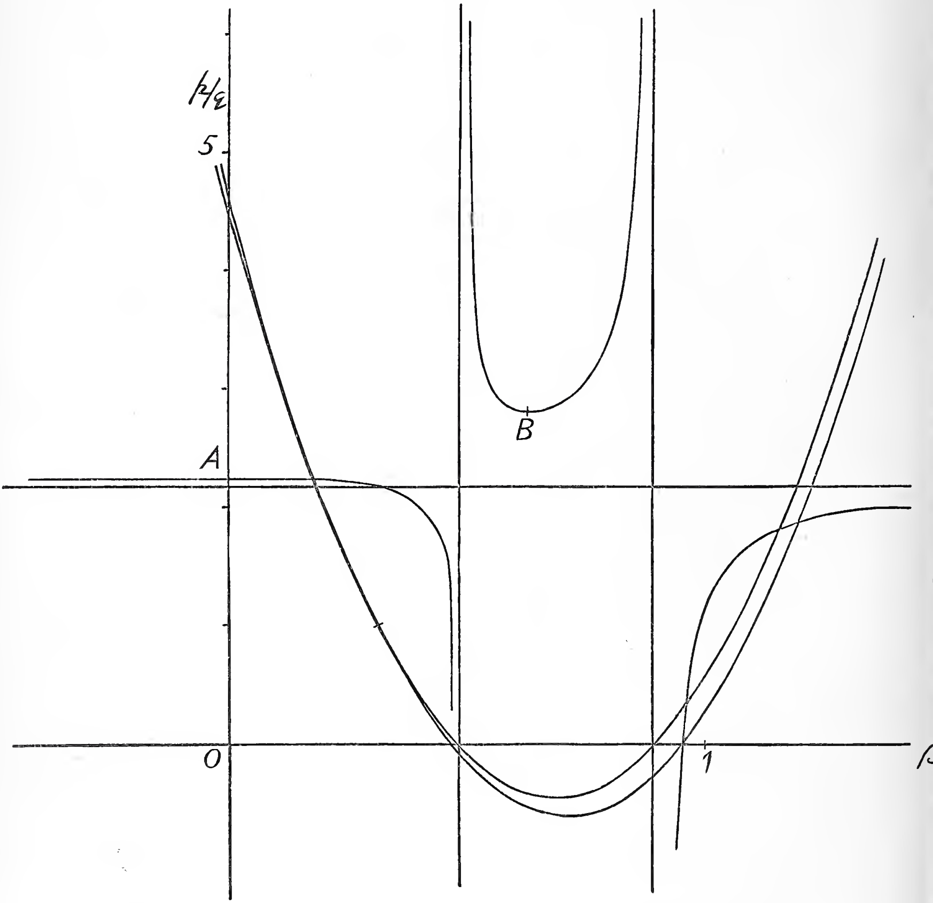
from which, at once,

$$-\frac{p}{q}=\frac{b^2(c^2-a^2)(b-\beta)\left(\frac{ca}{c+a}-\beta\right)}{a^2(b^2-c^2)(a-\beta)\left(\frac{bc}{b+c}-\beta\right)}+\frac{r}{q}\,\frac{c^2(a^2-b^2)(c-\beta)\left(\frac{ab}{a+b}-\beta\right)}{a^2(b^2-c^2)(a-\beta)\left(\frac{bc}{b+c}-\beta\right)}.$$

In the further discussion of this equation we may neglect the last term (which is usually *very* much smaller than the preceding term, and becomes infinite for the same values of $\beta$). Its only noticeable effect is to *slightly* alter the values of $\beta$ for which $p/q$ vanishes. We therefore have, to a quite sufficient approximation,

$$\frac{p}{q} = 2\cdot1712 \frac{(b-\beta)\left(\frac{ca}{c+a}-\beta\right)}{(a-\beta)\left(\frac{bc}{b+c}-\beta\right)}$$

where the literal factors have been retained in the more important portion. The value of $\frac{p}{q}$ in terms of $\beta$ is thus seen to be a




numerical multiple of the ratio of the corresponding ordinates of two equal and similarly situated parabolas, whose vertices do not coincide. The first cuts the axis of $x$ at $b$ and $ca/(c+a)$, the second at $a$ and $bc/(b+c)$, so that the second lies wholly within the first while $y$ is negative. They intersect in the single point whose abscissa is $abc/(ab+bc+ca)$. These parabolas are shown in the cut opposite.

The values of $p/q$ are the ordinates of the chief curve. This has three asymptotes :—two parallel to $y$, and cutting $x$ at $a$ and $bc/(b+c)$ respectively; and the third at a constant distance, 2·1712, from the axis of $x$. Its maximum ordinates are given by the equation

$$0 = \frac{d}{dx} \frac{(b-x)\left(\frac{ca}{c+a} - x\right)}{(a-x)\left(\frac{bc}{b+c} - x\right)},$$

or

$$0 = (ab+bc+ca)x^2 - 2abc\,x .$$

Thus the maximum (at A in the cut) is on the axis of $y$; and the minimum (at B) corresponds to $x = 0·6321$. Their values are 2·228 and 2·816 respectively; and the ordinate of the point of intersection of the construction-parabolas lies midway between them.

Thus, since the minimum numerically exceeds the maximum, the curve has no ordinate intermediate to these values; and therefore no selection of real constants can make Van der Waals' equation applicable to a liquid in which the pressure, required to reduce its volume by 10 per cent., exceeds that required for a 5 per cent. reduction, in any ratio between 2·228 and 2·816.

Moreover, in accordance with what has been said above about the term $A/v^2$, it is only while the ratio of pressures exceeds the higher of these limits that this term represents a pressure, and not a tension. For the graph of $A/q$ in terms of $\beta$ is easily seen to be a rectangular hyperbola whose asymptotes are parallel to the axes; cutting $x$ at $bc/(b+c)$, and $y$ at $b^2c^2/(b^2-c^2)$. The curve cuts $x$ at $b$, and so its ordinates are positive from $bc/(b+c)$ to $b$, *only*.

## Note on Magnetic Induction in Nickel Tubes. By Professor C. G. Knott, D.Sc., and A. Shand, Esq.

(Read June 18, 1894.)

This paper is a continuation of a paper communicated to the Society in July of last year, the publication of which was withheld until the whole subject could be treated as one. In the previous paper we discussed the magnetic induction in Iron and Steel Tubes. These tubes, and the nickel tubes now under discussion, were constructed primarily for the purpose of investigating their changes of volume under magnetisation;* but it seemed advisable to study as many of their magnetic properties as possible.

In the meantime an interesting paper by Professor Grotrian on "The Magnetisation of Hollow and Solid Iron Cylinders" has been published in Wiedemann's *Annalen* (Band 50, 1893). His tubes were considerably shorter than ours, and had much thinner walls. Many of the results obtained are, however, very similar. In a recent paper (Wiedemann's *Ann.*, Band 51, 1894) Dr H. du Bois discusses the most important of these results as illustrative of self-demagnetisation in magnetic bodies whose linear dimension is not very great compared to their breadth.

This mode of regarding the subject is a very suggestive one, and seems sufficient as an explanation of the broad result that in weak fields the magnetic moment of a hollow bar is equal to the magnetic moment of a solid bar of the same length and breadth. Thus, with the six iron bars, five of which were hollow, we obtained in a field of 20 C. G. S. units total magnetic inductions proportional to the numbers

665, 700, 702, 709, 703, 724,

where the first refers to the tube of widest bore, and so on in order of diminishing bore to the last, which refers to the solid bar. Similarly

* *Proc. Roy. Soc. Edin.*, vol. xix. pp. 85, 249, 1892.

for the steel tubes in the same field we obtained the total inductions proportional to

590, 604, 570, 605, 593.

In general, then, in low fields, the magnetic inductions in iron or steel tubes of the same material and external dimensions tend to equality, being independent of the size of the bore. In other words, the apparent average permeability in low fields is inversely as the cross section of the metal.

In very high fields again, the magnetic moments are proportional to the cross section of the metal, or the apparent average permeabilities tend to equality.

These few results are here introduced to facilitate a comparison with the results for nickel.

In our experiments four nickel bars were used, three of which were hollow. All were of the same length (47 cm.) and the same external diameter (4·2 cm.). The internal diameters and the areas of section of the metal were as follows :—

| Tube. | Diameter of Bore in cm. | Area of Section of Metal in cm.$^2$. |
|---|---|---|
| I. . . | 2·543 | 8·776 |
| II. . . | 1·586 | 11·879 |
| III. . . | 0·692 | 13·478 |
| IV. . . | 0 | 13·854 |

No. I. nickel corresponds fairly well, as regards its various relative dimensions, with No. II. of Iron or Steel; and No. III. nickel corresponds with No. V. of Iron or Steel. The tubes were all cut from the same original bar of rolled nickel; and the solid cylinder (No. IV.) from another bar rolled from the same original casting.*

In high fields, the result obtained with the nickel is the same as

* The long bars were supplied to order by Henry Wiggin & Co., Birmingham, and the tubes were turned and bored by Aitken & Allan, Edinburgh.

in the case of the iron and steel, the apparent average permeabilities being equal,—above 16 in a field of 585 C. G. S. units.

In low fields, the results obtained with the nickel tubes do not quite agree with those obtained with the iron and steel. There are indications, however, that somewhat similar conditions are fulfilled, as the following numbers may serve to show :—

*Induction in Low Fields.*

| Field. | Tube I. | Tube II. | Tube III. | Tube IV. |
|---|---|---|---|---|
| 4 | 85 | 113·6 | 103·6 | ... |
| 7 | 179 | 234 | 229 | 207 |
| 22 | 850 | 935 | 1050 | 1043 |
| 30 | 1210 | 1340 | 1490 | 1486 |
| 585 | 4600 | 6000 | 6900 | 6920 |

Thus, in sufficiently low fields, No. III. has a distinctly greater induction than No. IV., and No. II. than No. III.; but there is no indication of No. I. similarly exceeding No. II. The tendency seems to be in the other direction; for the induction in No. I. is proportionately smaller than that in No. II., the lower the field.

There are several reasons why the explanation mentioned above should fail to apply to the case of the nickel.

The mathematical theory of magnetic induction is based upon certain assumptions which are approximately realised when iron and steel are subjected to small magnetising forces. Thus, in iron tubes, the residual magnetism is barely appreciable in low fields, and the magnetisation is proportional to the magnetising force. Hence the relation given by theory is fairly applicable, namely,

$$I = \frac{k}{1 + kP} H ,$$

where I is the magnetisation, H the field, $k$ the susceptibility, and P the "demagnetising factor," as Du Bois terms it—a quantity whose value depends on the form of the body. Its value can be calculated for the ellipsoid, and is $\frac{4}{3}\pi$ in the case of the sphere. It vanishes for very elongated bodies, like wires. In such cases, when P is negligible, the ratio I/H measures the susceptibility.

When, however, the magnetised body is not very elongated, the value of P becomes appreciable; and Lord Rayleigh has shown how, by a simple graphic construction, we can pass from the magnetisation curve of a long thin wire to that of an oblate spheroid of any eccentricity. Now, the susceptibility of iron in vanishingly low fields has a value of about 30 or 40. Hence, in bars whose length is not more than 12 times the breadth, the ratio I/H is a measure rather of 1/P than of $k$. For this condition of things we have approximately

$$PI = H.$$

If $a$ is the area of section of the metal of the tube or bar, the magnetic moment is

$$Ia = H\frac{a}{P}.$$

But this is practically the same for all the tubes; consequently the demagnetising factor is proportional to the area of section of the metal.

If we try to apply the same reasoning to the case of the nickel tubes, we are met by two difficulties. In the first place, there is a considerable residual magnetism even in the lowest fields—as much as 52 per cent. in field 4. [This residual magnetism has a well-marked maximum—66 per cent. in the tube of widest bore—in a field of 25 or 30, just a little lower than the field for which I/H has a maximum value (Wiedemann's *Wendepunkt*).] Then, in the second place, the susceptibility of nickel in vanishingly low fields is probably not greater than 5 or 6. Thus we cannot hope to find the relation

$$I = \frac{k}{1 + kP}H$$

even approximately applicable; and still less can we regard the ratio I/H as measuring the reciprocal of the demagnetising factor.

Experimentally there is a further source of discrepancy in the results; for it is very difficult to get nickel (which has once been magnetised) into a magnetically "neutral" condition by the process of reversals.

Rowland gives 17·6 as the maximum value for $k$, the true susceptibility. The apparent maximum susceptibilities for the four nickel bars were found as follows :—

$$k' = 6{\cdot}6\,,\quad 5{\cdot}4\,,\quad 5{\cdot}2\,,\quad 5{\cdot}1\,.$$

Assuming the relation $k' = k/(1 + k\mathrm{P})$, we get

$$\mathrm{P} = \frac{1}{k'} - \frac{1}{k}\,,$$

and find for the four corresponding values of P the numbers

$$\mathrm{P} = {\cdot}095\,,\quad {\cdot}128\,,\quad {\cdot}135\,,\quad {\cdot}139\,.$$

The areas of section of the metal are in the ratios of the numbers

$${\cdot}095\,,\quad {\cdot}127\,,\quad {\cdot}145\,,\quad {\cdot}149\,.$$

To satisfy the law that the demagnetising factor is proportional to the cross-section, these corresponding numbers should be in the same ratio, each to each. The first two pairs are in accord; and so are the third and fourth pairs. But otherwise there is an obvious discrepancy. Nevertheless, when we consider the numerous probable sources of discrepancy between theory (admittedly approximate) and experiment, it is matter rather of surprise that there should be any concordance.

## Note on the Volume Changes which accompany Magnetisation in Nickel Tubes. By Professor C. G. Knott, D.Sc., F.R.S.E., and A. Shand, Esq.

(Read July 2, 1894.)

In the note which recorded the first observation of the volume effects of magnetisation,* the nickel tube experimented with was formed by rolling up a sheet of ordinary commercial nickel to the convenient size.

We are now able to record the preliminary results obtained with the nickel tubes described in the preceding Note on Magnetic Induction. Broadly speaking, the behaviour of the nickel resembles the behaviour of certain of the Iron Tubes as described in previous communications.†

The volume changes were measured in exactly the same way as formerly described, namely, by the motion of the liquid meniscus in a capillary tube which passed through the stopper of the metal tube, and was in continuous communication with its interior. The decrease of volume in the tube of widest bore (No. I.), when the field was 600 and the corresponding average magnetisation 700, was so large that it had to be measured with the naked eye. The meniscus moved outwards through a distance of 3 centimetres, which corresponded to a volume change of 2·4 cubic millimetres. This, with a total internal volume of 224·47 cubic centimetres, gives a dilatation of fully $-10^{-5}$. With the tube of intermediate bore (No. II.) the volume change in the same field was ·63 cubic millimetres, corresponding to a dilatation of a little less than $-10^{-5}$. Finally, with the tube of narrowest bore (No. III.) the volume change in field 600 was ·42 cubic millimetres, giving a dilatation of $-2{\cdot}3 \times 10^{-5}$. These values are all considerably greater than the

* "On the Effect of Longitudinal Magnetisation on the Interior Volume of Iron and Nickel Tubes. By Professor C. G. Knott (*Proc. Roy. Soc. Edin.*, vol. xviii., 1891).

† *Proc. Roy. Soc. Edin.*, vol. xix. pp. 85, 249, 1892; see also *Brit. Ass. Reports* (Edinburgh Meeting), 1892.

values obtained with the iron tubes under similar conditions (see vol. xix. p. 251).

The following table shows the more striking features of the volume changes of the three nickel tubes in various fields. Under each heading of field is a column containing the corresponding cubical dilatations for the three tubes. The volume changes can be easily calculated by multiplying the dilatations by the volumes of bore, which are given in the second column:—

*Table of Cubical Dilatations* $\times 10^7$.

| Tube. | Volume c.c. | Field. | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|
| | | 10 | 15 | 25 | 35 | 62 | 102 | 150 | 200 | 400 | 600 |
| I. | 224·47 | −0·5 | 0 | +0·8 | 0 | −9 | −24 | −47 | −67 | −98 | −110 |
| II. | 87·58 | ... | −0·7 | 0 | +1 | 0 | −10 | −35 | −53 | −79 | −87 |
| III. | 17·88 | ... | ... | +1·0 | +2·5 | +9 | 0 | −36 | −84 | −207 | −236 |

In the following table a direct comparison is made between the dilatations and the average intensities of magnetisation in the metal of the tubes. For in all probability it is the magnetisation rather than the magnetising force that is essentially involved. The magnetisations were obtained directly from the experiments described in the preceding Note on the Magnetic Induction in Nickel Tubes (p. 277):—

*Cubical Dilatations* $\times 10^7$.

| Tube. | Intensity of Magnetisation. | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | 50 | 80 | 150 | 230 | 300 | 340 | 410 | 500 | 600 | 700 |
| I. | −·3 | 0 | +0·8 | 0 | −3·5 | −7 | −14 | −27 | −55 | −110 |
| II. | ... | −·7 | 0 | +1 | +1 | 0 | −4·5 | −19 | −48 | −87 |
| III. | ... | ... | +2 | +7·5 | +9 | +7·5 | 0 | −32 | −125 | −236 |

From these tables certain broad conclusions may at once be drawn.

1. In high fields and with high magnetisations there is decrease of volume in the interior of all three tubes; but, below a certain moderate field, the dilatation is positive. The field and magnetisation at which this change of sign occurs are highest for the tube of thickest wall (III.) and lowest for the tube of thinnest wall (I.).

2. With Tubes I. and II. the dilatation is, however, negative in still lower fields; and the field and magnetisation at which this other change of sign occurs are higher for the tube of thicker wall (II.). Down to the lowest field in which a measurable change of volume for Tube III. was obtained, there was no evidence of a negative dilatation. But, as this lowest field was only 21·4 (whereas it was possible to measure changes of volume in Tube I. in as low a field as 4·7), it is impossible to say anything definite regarding the behaviour of Tube III. in very low fields.

3. The connection that is here indicated between the thickness of the wall and the field in which the dilatation changes sign, hints at a penetration of effect through the walls as the field increases in strength.

4. It is curious to note that the dilatations in the tube of intermediate bore are, as a rule, numerically smaller than the corresponding dilatations in the tube of widest bore, and yet the tube of narrowest bore gives by far the greatest values for the same quantity. This is not what our experience with the iron tubes would have led us to expect.

From experiments on different specimens of nickel strips and wires, Mr Bidwell * found for the elongations in field 600 the values $-107 \times 10^{-7}$ and $-240 \times 10^{-7}$. Assuming that our nickel tubes have similar elongations, we might calculate the elongations transverse to the lines of magnetisation. But such a calculation, though plausible enough in the case of the very thin-walled tube discussed in the earliest paper of 1891, is clearly quite out of the question here. In all probability the elongations of tolerably thick bars and tubes will differ materially from the elongations of wires and thin strips.

* See his Paper "On the Changes Produced by Magnetisation, &c.," *Phil. Trans.*, vol. clxxix., 1888, A, pp. 205–230.

## Note on Professor Cayley's Proof that a Triangle and its Reciprocal are in Perspective. By Thomas Muir, LL.D.

(Read April 4, 1892.)

The vertices of the original triangle being

$$(\alpha_1, \beta_1, \gamma_1), \quad (\alpha_2, \beta_2, \gamma_2), \quad (\alpha_3, \beta_3, \gamma_3),$$

the conic being

$$x^2 + y^2 + z^2 = 0,$$

and $A_1, B_1, C_1, \ldots$ being defined by the nine equations

$$A_1 = \frac{|\beta_2\gamma_3|}{|\alpha_1\beta_2\gamma_3|}, \quad \ldots\ldots\ldots$$

involved in the single matrical equation

$$\begin{vmatrix} \alpha_1 & \beta_1 & \gamma_1 \\ \alpha_2 & \beta_2 & \gamma_2 \\ \alpha_3 & \beta_3 & \gamma_3 \end{vmatrix}^{-1} = \begin{vmatrix} A_1 & A_2 & A_3 \\ B_1 & B_2 & B_3 \\ C_1 & C_2 & C_3 \end{vmatrix},$$

the equations of the lines joining the corresponding angles of the two triangles are found to be

$$\left.\begin{array}{l} (B_1\gamma_1 - C_1\beta_1)x + (C_1\alpha_1 - A_1\gamma_1)y + (A_1\beta_1 - B_1\alpha_1)z = 0 \\ (B_2\gamma_2 - C_2\beta_2)x + (C_2\alpha_2 - A_2\gamma_2)y + (A_2\beta_2 - B_2\alpha_2)z = 0 \\ (B_3\gamma_3 - C_3\beta_3)x + (C_3\alpha_3 - A_3\gamma_3)y + (A_3\beta_3 - B_3\alpha_3)z = 0 \end{array}\right\};$$

and what is required, of course, is to show that these lines must meet in a point; *i.e.*, that

$$\begin{vmatrix} B_1\gamma_1 - C_1\beta_1 & C_1\alpha_1 - A_1\gamma_1 & A_1\beta_1 - B_1\alpha_1 \\ B_2\gamma_2 - C_2\beta_2 & C_2\alpha_2 - A_2\gamma_2 & A_2\beta_2 - B_2\alpha_2 \\ B_3\gamma_3 - C_3\beta_3 & C_3\alpha_3 - A_3\gamma_3 & A_3\beta_3 - B_3\alpha_3 \end{vmatrix} = 0.$$

Professor Cayley effects this by a transformation of the elements of the determinant, and then by developing at length the determinant so found.*

Fortunately this tedious process is quite unnecessary, as the sum

* See *Quarterly Journal of Math.*, i. pp. 7–10, or *Collected Math. Papers*, iii. pp. 5–7.

of the elements in each column is manifestly zero. In fact we may say that *by definition*

$$B_1\alpha_1 + B_2\alpha_2 + B_3\alpha_3 = 0\,, \quad A_1\beta_1 + A_2\beta_2 + A_3\beta_3 = 0\,, \quad A_1\gamma_1 + A_2\gamma_2 + A_3\gamma_3 = 0$$
$$C_1\alpha_1 + C_2\alpha_2 + C_3\alpha_3 = 0\,, \quad C_1\beta_1 + C_2\beta_2 + C_3\beta_3 = 0\,, \quad B_1\gamma_1 + B_2\gamma_2 + B_3\gamma_3 = 0\,,$$

or, more fully, that

$$\begin{vmatrix} A_1 & A_2 & A_3 \\ B_1 & B_2 & B_3 \\ C_1 & C_2 & C_3 \end{vmatrix} \cdot \begin{vmatrix} \alpha_1 & \beta_1 & \gamma_1 \\ \alpha_2 & \beta_2 & \gamma_2 \\ \alpha_3 & \beta_3 & \gamma_3 \end{vmatrix} = \begin{vmatrix} 1 & 0 & 0 \\ 0 & 1 & 0 \\ 0 & 0 & 1 \end{vmatrix}.$$

# A Problem of Sylvester's in Elimination.

By Thomas Muir, LL.D.

(Read April 4, 1892.)

1. The problem in question with its solution appeared in the *Cambridge Mathematical Journal*, vol. ii. pp. 232–236, being given by Sylvester to show how his recently discovered "dialytic" method might be applied to ternary quadratics. The given equations are

$$\left.\begin{aligned} Ay^2 - 2C'xy + Bx^2 &= 0 \\ Bz^2 - 2A'yz + Cy^2 &= 0 \\ Cx^2 - 2B'zx + Az^2 &= 0 \end{aligned}\right\} ; \qquad (\alpha)$$

and Sylvester's solution consists in deducing from them three other equations

$$\left.\begin{aligned} C'z^2 + Cxy - A'zx - B'yz &= 0 \\ A'x^2 + Ayz - B'xy - C'zx &= 0 \\ B'y^2 + Bzx - C'yz - A'xy &= 0 \end{aligned}\right\} , \qquad (\beta)$$

and then eliminating $x^2$, $y^2$, $z^2$, $yz$, $zx$, $xy$; the result being

$$\begin{vmatrix} . & C & B & -2A' & . & . \\ C & . & A & . & -2B' & . \\ B & A & . & . & . & -2C' \\ A' & . & . & A & -C' & -B' \\ . & B' & . & -C' & B & -A' \\ . & . & C' & -B' & -A' & C \end{vmatrix} = 0 .$$

This determinant on being expanded, not without considerable trouble, is found to be equal to

$$2(ABC + 2A'B'C' - AA'^2 - BB'^2 - CC'^2)^2$$

*i.e.*,

$$2 \begin{vmatrix} A & C' & B' \\ C' & B & A' \\ B' & A' & C \end{vmatrix}^2 ;$$

so that the desired resultant comes out finally in the simple form

$$\begin{vmatrix} A & C' & B' \\ C' & B & A' \\ B' & A' & C \end{vmatrix} = 0,$$

the determinant in which (Δ, say) is nothing more nor less than the discriminant of the quadric

$$Ax^2 + By^2 + Cz^2 + 2A'yz + 2B'zx + 2C'xy.$$

2. It is impossible to note a result like this without the immediate uprise, in one's mind, of two questions, viz., (1) Is there no simpler way of obtaining the eliminant by means of the dialytic method? (2) How comes the given problem to be connected with the finding of the discriminant of a quadric? The object of the present short paper is to contribute towards the answering of these questions.

3. As regards the first of them, it is clear at the outset that if we are to avoid a determinant of high order, like Sylvester's, we must not retain in the same equation terms in $xy$, $yz$, or $zx$ along with terms in $x^2$, $y^2$, or $z^2$, but must aim at obtaining a set of equations involving only one of these two triads. Now, as Sylvester's derived set of equations may be got from the original set by looking upon A, B, C as the unknowns in the latter and solving accordingly, it is suggested to us to write the given equations in the form

$$\left.\begin{array}{rcl} Bx^2 + Ay^2 \phantom{+ Bz^2} & = & 2C'xy \\ Cy^2 + Bz^2 & = & 2A'yz \\ Cx^2 \phantom{+ Cy^2} + Az^2 & = & 2B'zx \end{array}\right\},$$

and, as it were, solve for $x^2$, $y^2$, $z^2$. This procedure leads to the equations

$$\left.\begin{array}{r} CC'xy - AA'yz + BB'zx = BCx^2 \\ CC'xy + AA'yz - BB'zx = CAy^2 \\ -CC'xy + AA'yz + BB'zx = ABz^2 \end{array}\right\} \qquad (\gamma)$$

This set (γ) resembles Sylvester's; and, we may note in passing, from the two taken together we have the resultant

$$\begin{vmatrix} BC & . & . & AA' & -BB' & -CC' \\ . & CA & . & -AA' & BB' & -CC' \\ . & . & AB & -AA' & -BB' & CC' \\ A' & . & . & A & -C' & -B' \\ . & B' & . & -C' & B & -A' \\ . & . & C' & -B' & -A' & C \end{vmatrix} = 0 ,$$

where, of course, the determinant is no simpler than Sylvester's. That it leads to the same result is easily made clear by first multiplying the 4th, 5th, and 6th rows by BC, CA, AB respectively, and then dividing the first three columns by BC, CA, AB respectively. We thus obtain

$$ABC \times \begin{vmatrix} 1 & . & . & A' & -B' & -C' \\ . & 1 & . & -A' & B' & -C' \\ . & . & 1 & -A' & -B' & C' \\ A' & . & . & BC & -CC' & -BB' \\ . & B' & . & -CC' & CA & -AA' \\ . & . & C' & -BB' & -AA' & AB \end{vmatrix} ;$$

thence

$$ABC \times \begin{vmatrix} 1 & . & . & A' & -B' & -C' \\ . & 1 & . & -A' & B' & -C' \\ . & . & 1 & -A' & -B' & C' \\ . & . & . & BC \ -A'^2 & A'B' \ -CC' & C'A' \ -BB' \\ . & . & . & A'B' \ -CC' & CA \ -B'^2 & B'C' \ -AA' \\ . & . & . & C'A' \ -BB' & B'C' \ -AA' & AB \ -C'^2 \end{vmatrix} ,$$

and finally

$$ABC \cdot \begin{vmatrix} A & C' & B' \\ C' & B & A' \\ B' & A' & C \end{vmatrix}^2 .$$

4. It is of importance to notice that since any one of the equations ($\gamma$) may also be got from the equations of the original set ($\alpha$) by mere addition or subtraction of multiples, *e.g.*,

$$\gamma_1 = \tfrac{1}{2}(-C\alpha_1 + A\alpha_2 - B\alpha_3),*$$

* Of course there are similar operational equations for the obtaining of $\gamma_2$ and $\gamma_3$, because any one of the original equations is derivable from another of

the dialytic method cannot be applied to the six equations ($\alpha$) and ($\gamma$). In fact, these sets must, for dialytic purposes, be looked on as identical.

5. But now taking ($\beta$), and eliminating $xy$, $yz$, $zx$ by means of ($\alpha_1$), ($\alpha_2$), ($\alpha_3$), we obtain

$$A'C(A'C' - BB')x^2 + B'C(B'C' - AA')y^2 + C'(AA'^2 - 2A'B'C' + BB'^2)z^2 = 0$$

Similarly

$$A'(BB'^2 - 2A'B'C' + CC'^2)x^2 + B'A(B'A' - CC')y^2 + C'A(C'A' - BB')z^2 = 0 \qquad (\delta).$$

and

$$A'B(A'B' - CC')x^2 + B'(CC'^2 - 2A'B'C' + AA'^2)y^2 + C'B(C'B' - AA')z^2 = 0$$

From these we have

$$\begin{vmatrix} C(A'C' - BB') & C(B'C' - AA') & AA'^2 - 2A'B'C' + BB'^2 \\ BB'^2 - 2A'B'C' + CC'^2 & A(B'A' - CC') & A(C'A' - BB') \\ B(A'B' - CC') & CC'^2 - 2A'B'C' + AA'^2 & B(C'B' - AA') \end{vmatrix} = 0\,;$$

or, multiplying the columns by A, B, C, and dividing the rows by C, A, B respectively,

$$\begin{vmatrix} A(A'C' - BB') & B(B'C' - AA') & AA'^2 - 2A'B'C' + BB'^2 \\ BB'^2 - 2A'B'C' + CC'^2 & B(B'A' - CC') & C(C'A' - BB') \\ A(A'B' - CC') & CC'^2 - 2A'B'C' + AA'^2 & C(C'B' - AA') \end{vmatrix} = 0,$$

the determinant in which is equal to

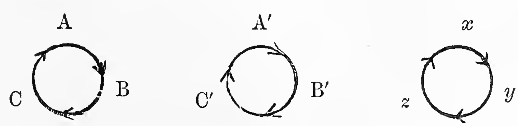
$$\begin{vmatrix} C'^2 - AB & AA' - B'C' & BB' - C'A' \\ BB' - C'A' & CC' - A'B' & A'^2 - BC \\ AA' - B'C' & B'^2 - AC & CC' - A'B' \end{vmatrix} \times \begin{vmatrix} B' & A' & . \\ C' & . & A' \\ . & C' & B' \end{vmatrix},$$

that is,

$$\Delta^2 \times (-2A'B'C')\,.$$

6. Next, taking ($\beta_1$) and ($\gamma_3$), and eliminating $z^2$, we obtain a set of equations involving only $yz$, $zx$, $xy$, viz.,

them by changing the letters in accordance with the indications of the cycles




—a mode of derivation that naturally applies to the equations of every other set likewise.

$$\left.\begin{array}{l} A(BB'-A'C')yz + B(AA'-B'C')zx + C(C'^2-AB)xy = 0 \\ A(A'^2-BC)yz + B(CC'-B'A')zx + C(BB'-C'A')xy = 0 \\ A(CC'-A'B')yz + B(B'^2-CA)zx + C(AA'-C'B')xy = 0 \end{array}\right\} (\epsilon)$$

the resultant of which is immediately seen to be

$$ABC \,.\, \Delta^2 = 0.$$

7. Again, taking the same two equations viz. $(\beta_1)$, $(\gamma_3)$, and now eliminating $xy$, we obtain an equation involving $xz$, $yz$, $z^2$—that is to say, involving the simple unknowns $x$, $y$, $z$, the full set being

$$\left.\begin{array}{l} (BB'-A'C')x + (AA'-B'C')y + (C'^2-AB)z = 0 \\ (A'^2-BC)\,x + (CC'-B'A')y + (BB'-C'A')z = 0 \\ (CC'-A'B')x + (B'^2-CA)y + (AA'-C'B')z = 0 \end{array}\right\} (\zeta).$$

Here the resultant is in a still simpler form, viz,

$$\Delta^2 = 0 .$$

8. We have thus arrived at the resultant in three different ways by means of a determinant of the third order, viz.,

in § 5, where the unknowns are $x^2$, $y^2$, $z^2$;
in § 6, where the unknowns are $yz$, $zx$, $xy$;
and in § 7, where the unknowns are $x$, $y$, $z$.

There are other ways, however; and though none of them is any simpler than that of the preceding paragraph, it is nevertheless interesting to see them, especially when they are brought into comparison with those already obtained.

Thus, taking $(\beta_1)$, $(\beta_2)$, and eliminating $x^2$ and $z^2$ from $(\alpha_3)$, we have.

$$\left.\begin{array}{l} A(CC'-A'B')yz + (2A'B'C'-AA'^2-CC'^2)zx + C(AA'-B'C')xy = 0 \\ A(BB'-C'A')yz + B(AA'-B'C')zx + (2A'B'C'-BB'^2-AA'^2)xy = 0 \\ (2A'B'C'-CC'^2-BB'^2)yz + B(CC'-A'B')zx + C(BB'-C'A')xy = 0 \end{array}\right\} (\eta)$$

where it would be easy to show that the resultant takes the form

$$2A'B'C' \cdot \Delta^2 = 0 ;$$

indeed the determinant here and the determinant of § 5 are transformable into one another by changing rows into columns. It is much more important, however, having now got *two* sets of equations in $yz$, $zx$, $xy$, to compare the one with the other. Doing so, we find that any equation of the one set, $(\eta_1)$ say, differs from an equation in the other set, $(\epsilon_3)$ say, in only one term. Consequently, by subtraction there results

$$(2A'B'C' - AA'^2 - CC'^2 - BB'^2 + ABC)zx = 0,$$

$$\text{whence} \qquad \Delta = 0.$$

Here, for the first time, $\Delta$ does not appear in the *second power*.

9. Leaving now the subject of the modes of obtaining the eliminant, let us see how the given problem comes to be connected with the finding of the discriminant of a quadric.

Taking the quadric

$$Ax^2 + By^2 + Cz^2 + 2A'yz + 2B'zx + 2C'xy,$$

let us suppose it resolvable into real factors, viz.,

$$\alpha_1 x + \beta_1 y + \gamma_1 z \qquad \text{and} \qquad \alpha_2 x + \beta_2 y + \gamma_2 z;$$

then the coefficients of the three expressions are connected by the six relations

$$\begin{array}{ll} \alpha_1\alpha_2 = A, & \beta_1\gamma_2 + \beta_2\gamma_1 = 2A', \\ \beta_1\beta_2 = B, & \gamma_1\alpha_2 + \gamma_2\alpha_1 = 2B', \\ \gamma_1\gamma_2 = C, & \alpha_1\beta_2 + \alpha_2\beta_1 = 2C'. \end{array}$$

Substituting in the last three the values of $\alpha_2, \beta_2, \gamma_2$ obtainable from the first three, we have

$$\left.\begin{array}{l} C\beta_1^2 + B\gamma_1^2 = 2A'\beta_1\gamma_1 \\ A\gamma_1^2 + C\alpha_1^2 = 2B'\gamma_1\alpha_1 \\ B\alpha_1^2 + A\beta_1^2 = 2C'\alpha_1\beta_1 \end{array}\right\},$$

which are exactly the three equations of Sylvester's problem.

## Note on Dr Muir's Paper, "A Problem of Sylvester's in Elimination." By Professor Cayley.

(Received November 6, 1894.)

I in part reproduce this very interesting paper for the sake of a remark which appears to me important. I write $(a, b, c, f, g, h)$ in place of Muir's (A, B, C, A′, B′, C′), and take as usual (A, B, C, F, G, H) and K to denote $(bc - f^2,\ ca - g^2,\ ab - h^2,\ gh - af,\ hf - bg,\ fg - ch)$ and the discriminant $abc - af^2 - bg^2 - ch^2 + 2fgh$.

I then write

$$\begin{aligned} U &= bz^2 - 2fyz + cy^2, & P &= fx^2 + ayz - hzx - gxy, & L &= bcx^2 + afyz - bgzx - chxy, \\ V &= cx^2 - 2gzx + az^2, & Q &= gy^2 - hyz + bzx - fxy, & M &= cay^2 - afyz + bgzx - chxy, \\ W &= ay^2 - 2hxy + by^2, & R &= hz^2 - gyz - fzx + cxy, & N &= abz^2 - afyz - bgzx + chxy. \end{aligned}$$

The equations $U = 0$, $V = 0$, $W = 0$, imply $P = 0$, $Q = 0$, $R = 0$, but observe that P, Q, R are not the sums of mere numerical multiples of U, V, W; we in fact have identically

$$\begin{aligned} 2yzP &= -x^2U + y^2V + z^2W, \\ 2zxQ &= x^2U - y^2V + z^2W, \\ 2xyR &= x^2U + y^2V - z^2W. \end{aligned}$$

If then $U = 0$, $V = 0$, $W = 0$, we have also $P = 0$, $Q = 0$, $R = 0$, and we can from the six equations dialytically eliminate $x^2, y^2, z^2, yz, zx, xy$, thus obtaining a result, Determinant $= 0$, which is $K^2 = 0$; this is in fact Sylvester's process for the elimination.

But L, M, N are sums of mere numerical multiples of U, V, W, viz., we have

$$\begin{aligned} 2L &= -aU + bV + cW, \\ 2M &= aU - bV + cW, \\ 2N &= aU + bV - cW, \end{aligned}$$

so that the original equations $U = 0$, $V = 0$, $W = 0$ are equivalent to and may be replaced by $L = 0$, $M = 0$, $N = 0$.

Muir shows that we have identically

$$\begin{aligned} L - fP &= x(Ax + Hy + Gz), \\ M - gQ &= y(Hx + By + Fz), \\ N - hR &= z(Gx + Fy + Cz), \end{aligned}$$

where observe that the first of these equations is

$$\left.\begin{array}{r}(fx^2 - ayz)(bz^2 - 2fyz + cy^2)\\ -(fy^2 - byz)(cx^2 - 2gzx + az^2)\\ -(fz^2 - cyz)(ay^2 - 2hxy + bx^2)\end{array}\right\} = 2xyz(\mathrm{A}x + \mathrm{H}y + \mathrm{G}z)$$

and similarly for the second and third equations.

He thence infers that the elimination may be performed by eliminating $x, y, z$ from the equations

$$\begin{aligned}\mathrm{A}x + \mathrm{H}y + \mathrm{G}z &= 0\\ \mathrm{H}x + \mathrm{B}y + \mathrm{F}z &= 0\\ \mathrm{G}x + \mathrm{F}y + \mathrm{C}z &= 0\end{aligned}$$

viz., that the result is

$$\begin{vmatrix}\mathrm{A}, & \mathrm{H}, & \mathrm{G}\\ \mathrm{H}, & \mathrm{B}, & \mathrm{F}\\ \mathrm{G}, & \mathrm{F}, & \mathrm{C}\end{vmatrix} = 0, \text{ that is } \mathrm{K}^2 = 0 \text{ as before.}$$

The natural inference is that K being $=0$, the three linear equations in $(x, y, z)$ are equivalent to two equations giving for the ratios $x : y : z$ rational values which should satisfy the original equations $\mathrm{U}=0$, $\mathrm{V}=0$, $\mathrm{W}=0$: the fact is that there are no such values, but that K being $=0$, the three equations are equivalent to a single equation: for observe that combining for instance the first and second equations, these will be equivalent to each other if only $\frac{\mathrm{A}}{\mathrm{H}} = \frac{\mathrm{H}}{\mathrm{B}} = \frac{\mathrm{G}}{\mathrm{F}}$, that is, $\mathrm{AB} - \mathrm{H}^2 = 0$, $\mathrm{GH} - \mathrm{AF} = 0$, $\mathrm{HF} - \mathrm{BG} = 0$, which are $c\mathrm{K}=0$, $f\mathrm{K}=0$, $g\mathrm{K}=0$, all satisfied by $\mathrm{K}=0$; and similarly for the first and third, and the second and third equations. It will be remembered that the true form of the result is not $\mathrm{K}=0$ but $\mathrm{K}^2=0$, and this seems to be an indication that the three equations should be, as they have been found to be, equivalent to a single equation.

The problem may be further illustrated as follows: instead of the original equations $\mathrm{U}=0$, $\mathrm{V}=0$, $\mathrm{W}=0$, consider the like equations with the inverse coefficients (A, B, C, F, G, H), viz.,

$$\begin{aligned}\mathrm{B}z^2 - 2\mathrm{F}yz + \mathrm{C}y^2 &= 0\,,\\ \mathrm{C}x^2 - 2\mathrm{G}zx + \mathrm{A}z^2 &= 0\,,\\ \mathrm{A}y^2 - 2\mathrm{H}xy + \mathrm{B}x^2 &= 0\,,\end{aligned}$$

so that the result of the elimination should be

$$(ABC - AF^2 - BG^2 - CH^2 + 2FGH)^2 = 0 .$$

Here considering in connection with the triangle $x=0$, $y=0$, $z=0$ (say the vertices hereof are the points A, B, C), the conic

$$(a, b, c, f, g, h)(x, y, z)^2 = 0$$

the first equation represents the pair of tangents from the point A to the conic, the second the pair of tangents from the point B to the conic, and the third the pair of tangents from the point C to the conic. The first and second pairs of tangents intersect in four points, and if one of the third pair of tangents passes through one of the four points, then it is at once seen that the conic must touch one of the sides $x=0$, $y=0$, $z=0$ of the triangle, viz., we must have $bc - f^2 = 0$, $ca - g^2 = 0$, or $ab - h^2 = 0$. But we have $a = BC - F^2$, &c., or writing $K_1 = ABC - AF^2 - BG^2 - CH^2 + 2FGH$, then these equations are $K_1A = 0$, $K_1B = 0$, $K_1C = 0$, all satisfied by $K_1 = 0$. We may regard $K_1 = 0$ as the condition in order that the conic $(a, b, c, f, g, h)(x, y, z)^2 = 0$ may be a *point-pair:* the analytical reason for this is not clear, but we see at once that if the conic be a point-pair, then the three pairs of tangents are the lines drawn from the points A, B, C respectively to the two points of the point-pair, so that the three pairs of tangents have in common these two points. Regarding $K_1 = 0$ as the condition in order to the existence of a single common point, and recollecting that the true result of the elimination is $K_1{}^2 = 0$, the form perhaps indicates what we have just seen is the case, that there are in fact two common points of intersection: but at any rate the foregoing geometrical considerations lead to $K_1 = 0$, as the condition for the coexistence of the three equations.

I remark in conclusion that I do not know that there is any general theory of the case where a result of elimination presents itself in the form $\Omega^2 = 0$, as distinguished from the ordinary form $\Omega = 0$.

## The Reproduction of the Edible Crab (*Cancer pagurus*). By Gregg Wilson, M.A., B.Sc., Natural History Laboratory, University of Edinburgh. *Communicated by* Professor J. C. Ewart, M.D., F.R.S.

(Read March 19, 1894.)

During the last two winters I have had considerable opportunity of making observations on the reproduction of the edible crabs of the Northumberland coast; and though my results are still incomplete, and some of them, as, for instance, those relating to the size of sexually mature crabs, are chiefly of local importance, I think the meagreness of past work on the subject, and its practical importance in connection with fishery legislation, justify me in publishing some account of what I have seen. I reserve for a subsequent paper many details, the full significance of which it is impossible to understand without further observation.

Male and female crabs can very readily be distinguished from one another. As they lie back upwards, the well-arched or "rounded" shell of the females is enough to enable fishermen to separate them from the flatter males; and even the size of the great claws in relation to the length of the shell is, I have observed, a sufficient characteristic: in the male the circumference of the claws is greater than the length of the cephalo-thorax, while in the female it is considerably less.*

* In a few crabs taken at random the measurements were as follows:—

| | Breadth. | Length. | Right Claw. | Left Claw. |
|---|---|---|---|---|
| Male . . . | 6 in. | $3\frac{3}{4}$ in. | $4\frac{1}{4}$ in. | $4\frac{3}{4}$ in. |
| ,, . . . | $5\frac{3}{4}$ ,, | $3\frac{7}{8}$ ,, | $4\frac{1}{8}$ ,, | $4\frac{1}{4}$ ,, |
| ,, . . . | $5\frac{1}{2}$ ,, | $3\frac{5}{8}$ ,, | 4 ,, | $3\frac{5}{8}$ ,, |
| Female . . | 8 ,, | $5\frac{1}{4}$ ,, | 4 ,, | $4\frac{1}{8}$ ,, |
| ,, . . | $7\frac{5}{8}$ ,, | $5\frac{1}{8}$ ,, | 4 (almost) | 4 ,, |
| ,, . . | $6\frac{3}{4}$ ,, | $4\frac{1}{2}$ ,, | $3\frac{5}{8}$ in. | $3\frac{3}{4}$ ,, |
| ,, . . | $6\frac{1}{2}$ ,, | $4\frac{1}{4}$ ,, | $2\frac{3}{4}$ ,, | $3\frac{1}{2}$ ,, |

Then the view of the ventral surface makes the sex equally plain: the female has a broad up-tucked abdomen or "flap," margined with long hairs, and bearing on the side next the body four pairs of modified, biramous, hairy appendages; the male exhibits a much narrower and more pointed flap, that bears two pairs of hard, channelled styliform appendages, which are adapted for the transmission of milt, just as those of the female are suitable for giving attachment to the spawn. Under the flap, too, opposite the third pair of walking-legs, the female has two large and very distinct apertures, through which the roe is extruded; the male has its genital apertures at the ends of soft funnel-shaped processes from the basal joints of the fifth pair of walking-legs.

But if the sex is easily determined, the same cannot be said of the periods of sexual activity. The females, after spawning, carry their ova under the flap for several months; and so fishermen at certain places find the "berried crab" during a great part of the year, and conclude that the spawning occurs at any season. On the other hand, at some places it is a rare thing to find a crab in berry, and a chance occurrence leads to a generalisation as to the spawning-time, with the result that, in such districts, almost any month in the year may be stated as the time of spawning.

I have proceeded to settle the matter by examining the reproductive organs of large numbers of crabs, and by keeping crabs in "hullies," or crab-stores, and in boxes.

Following the first method, I ascertained that, towards the end of the year—in the last two or three months—and in January, the female crabs that were caught might be sorted into two lots: those that had recently cast, and were not preparing to spawn soon, and those that had *not* recently cast, and had well-developed ovaries. A third class exists at the time, namely, those that have recently spawned; but these conceal themselves so well, that I only obtained specimens from my hullies.

The soft crabs or recent casters have pale ovaries, that show no development of ova to the naked eye; the hard crabs have brilliant orange or scarlet ovaries, with ova distinctly visible, and often, because of their softness, seeming larger under the microscope than the tenser extruded ova taken from under a crab's flap.

The catch, then, from October to February, so far as it consists of mature female crabs,—and they form a very large proportion at that time,—is made up of caster crabs, which are worthless for trade purposes, and spawning crabs, which, of course, are specially valuable for the up-keep of the species, and might well be protected by law wherever there is evidence of a decline in the fishery.

What, from a scientific point of view, is equally worthy of note, is that it seems as if the spawning took place only every second year of the crab's life. As the casters have undeveloped roes in the autumn, it is not likely that they are prepared to spawn till late in the following year; and, on the other hand, the crabs that are hard and prepared to spawn in autumn probably carry their ova through a considerable part of the next year, and appear as casters with undeveloped roes in the following autumn.

At no time have I found ova undergoing segmentation within a crab; and the old idea, that fertilisation is internal, must be abandoned. F. Buckland and Spencer Walpole say in their "Report on the Crab and Lobster Fisheries" (1877): "The crab carries its ova during the early period of pregnancy inside its shell." Milt undoubtedly is passed by the male crabs into the body of the females; but it does not affect the roe before extrusion. It is received in flask-shaped *receptacula seminis*, that open off the oviducts quite near the genital apertures. They are well-valved, and seem to retain the motionless spermatozoa for long periods.

The ripe male is found in all the months in which I have examined crabs. There is a popular belief that milting takes place when the females have newly cast; and the idea is founded on the well-established fact that, when the females are casting in the rock-holes along our coasts, they are commonly watched over by hard males. But it is noteworthy that the sentry is sometimes an unripe male; and sometimes a female is found guarding a soft male. These instances, taken together with the fact that males are found fully ripe long after casting is over, lead me to doubt the accepted view. I have noticed, too, that while I have carried a male and female shorewards from one of their rocky holes, the hard claws of the male have very readily passed through the soft casing of the caster; and it has occurred to me that such damage would almost necessarily result to the female, if impregnation were to be effected *immediately* after exuviation.

A point that, I think, has hitherto escaped notice is that the sperms that are found in the vasa deferentia of the male crab are never free, but always in packets, which may be either globular or elongated and bolster-like. Usually I have found the milt contained in the *receptacula seminis* of females to consist of a paste of free sperms.

From crabs kept in confinement I have learned nothing as to the milting process; but several facts as to the time and mode of spawning have been established. In the middle of December 1891 a Cresswell fisherman found that eight out of about thirty crabs that had been left in his crab-store for nearly a month had become berried since he put them in. In November 1892 another Cresswell man put forty or fifty crabs into a hully; and after a few days, on taking them out, he found that two of them had new spawn on them. In December of the same year I myself put a number of crabs into a hully, and on the 16th of January 1893 I found that one of them had been berried for some time, and another was in the act of spawning. Lying on its back, with the flap well raised, it had a pool of spawn between its walking legs, and into this it plunged, time after time, the endopodites of the anterior abdominal appendages, which were then moved to and fro, so as to distribute a share of the semi-fluid mass to the other abdominal appendages. A considerable quantity of the extruded spawn lay on the floor of the hully, and was washed away by the next tide.

Again, on last Christmas day, four of a number of crabs that were being kept for me at Beadnell were found to have become berried.

I have not been able to ascertain how long the berries are carried by the mother crab. Various attempts have been made to keep the crabs that have become berried in confinement, but have failed; and as the conditions were far from natural, this is not surprising: one could not expect successful hatching in hullies that were dry for two or three hours every tide. In June of last year, and again in August, the ova of some crabs that had been found in spawn some time previously hatched out.

Little is known about the habits of the berried crabs. That they do not feed much is made probable by the small number that are taken in the creaves; and one case that has been brought to my notice suggests that perhaps they bury themselves under sand for a time: one was found well covered with sand in a pitcher that was brought up on a fisherman's line from a sandy bottom at Holy Island. It is not till May that they are got in considerable numbers, even at the parts of the coast where they are most abundant; so it would appear that it is only when the time for the hatching out of the spawn comes near that they begin to feed at all freely. They are almost invariably found on sand; and the fact that they are got congregated in certain districts indicates that there is migration * connected with either the spawning act or the hatching out of the ova.

A matter of as great importance, from an economic point of view, as the determination of the spawning time, is to ascertain the size of maturity. At present crabs that are under $4\frac{1}{4}$ inches are protected by law, and in the English Channel this is sufficient to ensure that all will reach the spawning size. In the North, however, it is otherwise: I have only comparatively rarely seen a mature female crab smaller than 6 inches, and usually $6\frac{1}{2}$ inches is the adult size. This conclusion, which is based on an examination of many ovaries, is confirmed by the general experience of the Northumberland fishermen, many of whom say they have never seen a berried crab less than 7 inches across, though a fishery officer was able to give me a shell measuring only $5\frac{7}{16}$ inches, as belonging to the smallest berried crab he had ever seen.† There is

* The whole subject of the migration of crabs has been much discussed: they are, in fact, fished offshore in the early months of the year, and inshore later on; but it is constantly urged that this is no proof of seasonal migration, and that a storm in summer is enough to stir up crabs well offshore, while in winter the inshore crabs may be buried. But a Beadnell man has got some definite results of labelling experiments for me: two crabs out of twelve that were marked and liberated at the shore in December 1892 were recovered in 1893,—one in March, $1\frac{1}{2}$ miles out; the other in April, fully 3 miles from the coast. Another, labelled in December, and put into the sea 2 miles out, was recovered in July close to the beach.

† To illustrate my method, I give an abstract of my notes on twenty-two hard (selected) female crabs that were examined on 5th October:—

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Of | 4 | from | $5\frac{3}{8}$ | inches to | 6 | inches | all were immature. |
| Of | 8 | ,, | $6\frac{1}{2}$ | ,, | 7 | ,, | 4 were immature.<br>4 were mature and ripe. |
| Of | 10 | ,, | 7 | ,, | $7\frac{3}{4}$ | ,, | all were mature and ripe. |

serious danger of the destruction of the crab fishery at some parts of our coast, owing to the number that are allowed to breed being inadequate to keep up the species.

Males are mature when much smaller. Most of those above $4\frac{3}{4}$ inches that I have examined have been ripe; and several smaller than $4\frac{1}{2}$ inches have proved to be adults. But even males, it seems to me, might with advantage be protected till they reach a higher limit than the legal $4\frac{1}{4}$ inches; for the low price brought by the small crabs is not the equivalent of the probable gain if they are returned to the sea. In one case a male of $4\frac{3}{8}$ inches measured $5\frac{1}{2}$ inches immediately after casting.

## On the Chemical Composition of Sea-Water.

By **John Gibson**, Ph.D.

(Read July 3, 1893.)

The question as to the perfect uniformity or otherwise of the chemical composition of sea-water is one of great difficulty and complexity. It is quite certain that, broadly speaking, great differences do not exist, so that a conclusion can only be arrived at by the application of methods of analysis of great refinement, and then only if the limits of experimental error inherent in these methods be satisfactorily determined.

Thus the late Prof. Dittmar, when discussing the results of the analytical work in his "Challenger" Reports (vol. i. p. 26), makes the following statement :—"When we compare the percentages of the several components with the respective means, we frequently meet with differences which lie decidedly beyond the probable limits of the analytical error ; hence the variations must be owing partly to natural causes. Unfortunately, whatever these causes may be, they must in their effect on the numbers be presumed to be, to a certain extent, of the nature of observational errors, and to this extent they are in our reports inseparably entangled with the analytical errors."

The fact is that direc chemical analysis, meaning thereby the quantitative determination of the several constituents of sea-water, has hardly reached that degree of perfection which is requisite. Moreover, the labour involved in making one full analysis of sea-water is very great, and a satisfactory conclusion would require a very large number of such analyses to be carried out with every refinement. As this is beyond our reach, it becomes a matter of great importance to arrive at some method whereby the existence of slight differences in chemical composition may be indicated and recorded in such a manner as to admit of comparison with each other, even though the method does not give us directly a perfectly full and clear knowledge of the nature of the differences which it indicates, but only their cumulative effect. I have described and applied a method of this nature, which consists simply in the determination of a certain ratio which is necessarily a constant, if the assumption be made that there is no appreciable difference in the chemical composition of sea-water in different localities.

This ratio, following the example of Dittmar, I have denoted D. Dittmar uses this letter to denote a ratio which varies with the temperature, though it is constant for any given temperature.

Thus he puts

$$D = \frac{{}_4S_t - {}_4W_t}{\chi}$$

Where

${}_4S_t$ = The specific gravity of sea-water at the temperature $t$ referred to pure water at 4° C. as standard.

$W_t$ = The specific gravity of water at the temperature $t$ referred to water at 4° C. as standard.

$\chi$ = The total halogen, calculated as chlorine, per kilo. of sea-water.

Dittmar determined the value of D for a mixture of "Challenger" sea-water samples at a number of different temperatures, and showed that with these mixtures the values of D for different temperatures when calculated by an equation $D = a + bt + ct^2$ were in very close approximation to those found experimentally.

He came to the conclusion that for a given temperature the excess of the specific gravity of sea-water over that of pure water is proportional to $\chi$, and it is obvious that if the assumption thus made be correct for all sea-waters, we may in every case calculate $\chi$ from the known density or the density from a known $\chi$. For

$$\chi = \frac{{}_4S_t - {}_4W_t}{D}$$

and

$${}_4S_t - {}_4W_t = D\chi.$$

Dittmar determined $\chi$ experimentally, by a special modification of Volhard's method, in 315 of the "Challenger" samples, and compared these with the theoretical values $\chi'$, calculated from Buchanan's observed specific gravities by means of the formula

$$\chi' = \frac{{}_4S_t - {}_4W_t}{D}.$$

The differences $\chi' - \chi$ between the theoretical values and those actually found are given in Table I. column 9 of his Report. It must be noted that in calculating $\chi'$, Dittmar added on 0·04 (water = 1000) to Buchanan's specific gravities in order to undo the correction to vacuo which these contain, and so bring the results into accord with his experimental conditions. These differences, as they stand in Dittmar's Report, greatly exceed the limits of the experimental errors to which Buchanan and Dittmar respectively admit their methods to be liable. Buchanan estimates the experimental error in his determination at not more than ·05 (water at 4° C. = 1000). This corresponds to ± ·035 in $\chi' - \chi$, and Dittmar calculates his errors in his determinations of $\chi$ at certainly not more than ± 0·03. Errors, therefore, of ± 0·065 might reasonably be expected. Now excluding, as Dittmar did, some five or six altogether abnormal values for $\chi' - \chi$, we find these differences ranging from + 0·245 to − 0·231. Errors of + 0·100 and upwards occurring frequently, while errors of − 0·100 are of so very frequent occurrence as to strike the eye at once on a cursory inspection of the table. Dittmar calculates that a constant correction of − 0·042 should be applied to these differences ($\chi' - \chi$) as they stand in his Report, and formulates the net result of his inquiry by putting

$$\chi' - \chi = -0{\cdot}042 \pm \delta\,,$$

"where $\delta$ is a variable quantity, of which the chances are even that it is less or greater than 0·06, and about 8 against 2 that it is less than 0·12." Very unfortunately Dittmar, for some reason or other, did not determine experimentally the density of those waters in which he determined $\chi$. Having failed to trace any connection between geographical distribution and the slight differences in chemical composition which he observed in the case of 77 "Challenger" samples, which he subjected to a full analysis, and having come to the conclusion that such differences, if any, were negligable, it is perhaps not to be wondered at that this task was not undertaken. One thing Dittmar actually did was to make an invaluable investigation into the thermal expansion of sea-water, which made it possible to reduce the specific gravity determinations made by Mr J. Y. Buchanan to a common standard temperature. The standard temperature adopted was 15·56° C. or 60° F.

Some years ago, at a meeting of this Society, I gave it as my opinion that these differences ($\chi' - \chi$) found by Dittmar should not be attributed merely to experimental errors, but in great measure to actual differences in the relative proportions of the saline constituents in the various samples, and stated further that certain oceanic areas appeared to be characterised by positive or negative values for these differences.

At that time, however, I was misled by a most unfortunate error of calculation in certain of my determinations of $\chi$, and was under the impression that differences similar to those of the "Challenger" samples could frequently be traced in samples collected near our own shores. This not only led me into certain erroneous speculations regarding the conditions obtaining in the North Sea, but prevented me from perceiving the full bearing of my results on the questions raised by the "Challenger" Report.

It would serve no useful purpose to enter into a detailed explanation of the somewhat extraordinary circumstances which combined to prevent me from detecting an error for which, after all is said and done, I am responsible.

I confine myself to stating that in those cases where in my Reports* samples of North Sea or Arctic water appear as giving values for $D = 1{\cdot}47$, or thereabout, a constant correction of $+0{\cdot}00447$, applied to the logarithms of the values $\chi$, will bring these cases into perfect agreement with the others. This does not apply to the samples collected in the Cattegat, the Sound, the Baltic, and the Norwegian fiords. The high values (D) for these samples remain unaffected, and are to be explained by their containing salts derived from the land, as stated in my Report.

The value 1·4556 for D, which I gave as characteristic of the Atlantic water round our shores, has recently been confirmed by Mr H. N. Dickson in a very able paper, to which I shall have occasion to refer again.

As used by me, D has a more restricted significance than that given to it by Dittmar. It is defined by putting

$$D = \frac{{}_0S_0 - 1000}{\chi},$$

where ${}_0S_0 =$ density of sea-water at 0° C. referred to that of pure water at the same temperature, $\chi =$ total halogen calculated as chlorine per kilo. of sea-water.

* Seventh Annual Report of the Fishery Board for Scotland (1888), p. 409.

The values for ${}_0S_0$ were determined by direct weighings of portions of the different sea-waters measured at 0° C. in a modified form of Sprengel tube, and then dividing the weights thus found by the weights of equal volumes of distilled water measured also at 0° C. in the same Sprengel tube.

All the weighings were fully corrected for the displaced air, the density of which was calculated from observations of the temperatures at the time of weighing, of the barometric height and of the wet and dry bulb thermometer.

The determinations of $\chi$ were made by Dittmar's modification of Volhard's method, exactly in the manner described by Dittmar in his "Challenger" Report.

The degree of accuracy of which this method is capable may be gathered from the following summary of my corrected results:—

In 122 samples of sea-water obtained from the Moray Firth, the Pentland Firth, the North Sea, and including seven surface-samples from high northern latitudes (62° N. lat. to 79° N. lat.), the following values for the ratio $\frac{{}_0S_0 - 1000}{\chi} = D$ were obtained:—

| | |
|---|---|
| Mean of 122 determinations, . . . | 1·4563 |
| Maximum ,, . . . | 1·4585 |
| Minimum ,, . . . | 1·4535 |

This corresponds to a range of differences $\chi' - \chi$ of from $+0·024$ to $-0·042$, according as we compare the effect of the mean value for D with the effect of the maximum or minimum values respectively in calculating $\chi$ from the density.

The first and most obvious bearing which this result has is, that whatever explanation may be given of the magnitude of the differences $\chi' - \chi$ in the "Challenger" Report, they cannot possibly be due to the method used by Dittmar in his determinations of $\chi$. This becomes the more clear when we remember that the values which Dittmar gives for $\chi$ are, generally speaking, the mean of two determinations, whereas mine are derived from single determinations.

If then they are due to experimental errors, we must attribute them almost entirely to Buchanan's specific gravity determinations. There is, however, another possible explanation. These differences might be due to alterations in the chemical composition of a number of the samples combined with the effect of evaporation, due to imperfect stoppering on others. Such an explanation would have the effect of invalidating, to a great extent, the labours of these eminent observers, but I am glad to say that it appears to me that this apprehension is unfounded.

Dittmar's estimate of the constant hydrometer error is based upon the assumption that his estimate of the value for D was applicable to all the samples which he examined. It must, however, be remembered that he determined this value for D not in a number of different samples, but in a mixture made from a number of different samples. Obviously if the value for D varied appreciably in samples from different localities, the result of his experimental determinations of the value for D would depend upon the selection which he made. Although there is, unfortunately, little or no information available on this point. I am pretty certain that his value was distinctly too high for many of the samples which he examined, and very much too low for others. Be this as it may, his method of reasoning undoubtedly led him to underestimate the importance of one part of his work. He made a direct experimental comparison between the results of his plunger method for the determination of specific gravity and those obtained by Buchanan's hydrometer method, and in making this comparison he used the identical hydrometer with which Buchanan worked on board the "Challenger." Now, Mr H. N. Dickson has recently stated, in the paper to which I have already referred, that he found a constant difference between the values for ${}_0S_0$ as determined by my method and those obtained by using a "Challenger" hydrometer, and reducing the observed value ${}_4S_t$ to ${}_0S_0$ by means of Dittmar's tables—the use of which tables he moreover shows introduces no appreciable error. Further, as Mr Dickson points out, the amount of this constant difference was identical with that observed by Dittmar in comparing his plunger method with Buchanan's hydrometer method.

Thus the result of direct experimental comparison between Buchanan's hydrometer method and two gravimetric methods, differing greatly from each other, was that Buchanan's hydrometers gave, in the case of waters having a density ${}_0S_0$ approximating to 1028, a result which is too low by 0·12.

Mr Dickson further showed that the difference observed by him varied with the density, being zero for observations made in pure water. To the value for this difference he gave the form

$$\Delta = \cdot 0042 \ ({}_0S_0 - 1).$$

Mr Dickson's observations were made with waters collected in the English Channel having values for ${}_0S_0$ near 1028. By applying to the results obtained with the particular hydrometer which he employed, when reduced to ${}_0S_0$, a constant correction of $+0\cdot 12$, and using these corrected results along with chlorine determination made by the Volhard-Dittmar method, he obtained values for D agreeing very closely indeed with those obtained by me.

His lowest value for D out of 42 determinations being 1·449 and his highest 1·458; the probable value for D in two sets of observations being 1·4548 and 1·4554 respectively.

A consideration of the bearing of these several investigations led me to attempt a recalculation of the values for $\chi' - \chi$ in a number of typical cases taken from the "Challenger" Reports, using Dittmar's values ($\chi$) for the chlorine per kilo., Buchanan's specific gravities (${}_4S_{15\cdot 56}$) reduced by Dittmar's tables to ${}_0S_0$ plus Dickson's correction of $+0\cdot 12$ and my value for D, viz. 1·4560. In almost every case the result was a correction of the values for $\chi' - \chi$ given in Dittmar's Report, Table I., of approximately $+0\cdot 100$.

This correction altogether alters the apparent significance of these values. The large number of samples which in Dittmar's Report are associated with values for $\chi' - \chi$, ranging from, say, $-\cdot 180$ to $-\cdot 030$, give, after applying this correction, values for $\chi'$ according within reasonable limits with the values for $\chi$ determined experimentally by Dittmar. On the other hand, those samples with values from zero to $+0\cdot 245$ in Dittmar's Report all show a difference between the calculated and the experimental result which distinctly, and, indeed, in many cases very greatly, exceeds the limits of observational error. I have also recalculated the values for D in a number of typical cases with the result that a large proportion of the "Challenger" samples give values for D approximating closely to that found to be characteristic of the sea-water derived from the Atlantic Ocean between 50° and 80° N. lat.

On the other hand, about one-third of the samples give values for D above 1·464, and ranging up to 1·483, while only some twelve or fifteen samples give values for D falling below 1·449. To put the matter in another light: In the case of more than two-thirds of the 315 "Challenger" samples with which Dittmar made determinations of $\chi$, there is to all appearance an entire absence of grounds for suspecting either that Buchanan's specific gravity determinations were inaccurate, or that appreciable evaporation or chemical change had taken place during the years which elapsed between their collection and their examination by Dittmar.

With regard to the remaining samples, we have no choice between assuming either an inaccuracy in the specific gravity determinations or some chemical change occurring between the time of their collection and their examination by Dittmar, or, finally, that they were originally of appreciably different chemical composition.

This latter assumption appears to me to have by far the greatest probability on experimental grounds alone, but this probability is increased when we consider the case in the light of a critical examination of the results of Dittmar's full analyses of 77 of the "Challenger" samples of sea-water and in connection with geographical and with vertical distribution. The full discussion of this side of the question I must reserve for a future communication, when I have completed all the necessary calculations, and have had time to consider each case separately. I have, however, made rough diagrammatic representations of the distribution of the "Challenger" waters giving normal and abnormal values for D and $\chi' - \chi$. The charts used for this purpose, showing the course of H.M.S. "Challenger," were kindly given to me by Dr John Murray. These I lay before the meeting.

## On a Theorem regarding the Difference between any Two Terms of the Adjugate Determinant. By Thomas Muir, LL.D.

(Read December 3, 1894.)

1. If we take the set of equations

$$\left.\begin{array}{l} a_1yzw + a_2xzw + a_3xyw + a_4xyz = 0 \\ b_1yzw + b_2xzw + b_3xyw + b_4xyz = 0 \\ c_1yzw + c_2xzw + c_3xyw + c_4xyz = 0 \\ d_1yzw + d_2xzw + d_3xyw + d_4xyz = 0 \end{array}\right\} ,$$

the eliminant of which is

$$|a_1b_2c_3d_4|,$$

and from the 1st, 2nd, and 3rd eliminate $xyw$ and $xyz$, from the 2nd, 3rd, and 4th eliminate $xyz$ and $yzw$, from the 3rd, 4th, and 1st eliminate $yzw$ and $xzw$, and from the 4th, 1st, and 2nd eliminate $xzw$ and $xyw$, we obtain the set

$$\left.\begin{array}{l} |a_1b_3c_4|y + |a_2b_3c_4|x = 0 \\ |b_2c_4d_1|z + |b_3c_4d_1|y = 0 \\ |c_3d_1a_2|w + |c_4d_1a_2|z = 0 \\ |d_4a_2b_3|x + |d_1a_2b_3|w = 0 \end{array}\right\} ,$$

the eliminant of which is clearly

$$|a_2b_3c_4|\cdot|b_3c_4d_1|\cdot|c_4d_1a_2|\cdot|d_1a_2b_3| - |a_1b_3c_4|\cdot|b_2c_4d_1|\cdot|c_3d_1a_2|\cdot|d_4a_2b_3|.$$

We are thus led to conclude that the first form of the eliminant is a factor of the second.

The attempt to prove this, and the investigation of the form of the quotient, have brought to light a new theorem in determinants, which promises to be of some considerable importance.

2. The theorem is to the effect that *the difference between any two terms of the adjugate determinant is divisible by the original determinant.*

Taking, for shortness' sake, the determinant of the 5th order

$$|a_1b_2c_3d_4e_5| \quad \text{or} \quad \Delta$$

with its adjugate

$$|A_1B_2C_3D_4E_5|,$$

let us first establish a few simple cases, in order that the principle at the basis of the demonstration may become familiar, and the law of formation of the quotient come gradually into evidence.

The simplest of all cases is where the two terms differ in only two of the elements,—for example, the terms $A_1B_2C_3D_4E_5$ and $A_1B_2C_3D_5E_4$. Here it is evident that

$$A_1B_2C_3D_4E_5 - A_1B_2C_3D_5E_4 = A_1B_2C_3 \cdot |D_4E_5| .$$

But $|D_4E_5|$, as a minor of the adjugate and of the 2nd order, is equal to $\Delta$ multiplied by the complementary minor of the corresponding minor in the original determinant. Hence

$$A_1B_2C_3D_4E_5 - A_1B_2C_3D_5E_4 = A_1B_2C_3 \cdot |a_1b_2c_3| \cdot \Delta .$$

Next, taking the case where three of the elements differ, we have

$$\begin{aligned} A_1B_2C_3D_4E_5 - A_1B_2C_4D_5E_3 &= A_1B_2C_3D_4E_5 - A_1B_2C_3D_5E_4 \\ &\quad + A_1B_2C_3D_5E_4 - A_1B_2C_4D_5E_3 , \\ &= A_1B_2C_3 \cdot |D_4E_5| - A_1B_2D_5 \cdot |C_3E_4| , \\ &= \{ A_1B_2C_3 \cdot |a_1b_2c_3| - A_1B_2D_5 \cdot |a_1b_2d_5| \} \cdot \Delta . \end{aligned}$$

And now, as if quite generally, let us take the terms $A_5B_1C_4D_3E_2$ and $A_2B_4C_1D_5E_3$. Directing our attention to the interchanges which must take place between indices of the former in order to transform it into the latter, we see that the intermediate stages of transformation may be

$$A_5B_4C_1D_3E_2 ,$$
$$A_2B_4C_1D_3E_5 .$$

Affixing each of these, first with the negative sign and then with the positive sign, to the difference in question, we have

$$\begin{aligned} A_5B_1C_4D_3E_2 - A_2B_4C_1D_5E_3 &= A_5B_1C_4D_3E_2 - A_5B_4C_1D_3E_2 \\ &\quad + A_5B_4C_1D_3E_2 - A_2B_4C_1D_3E_5 \\ &\quad + A_2B_4C_1D_3E_5 - A_2B_4C_1D_5E_3 , \\ &= A_5D_3E_2 \cdot |B_1C_4| - B_4C_1D_3 \cdot |A_2E_5| + A_2B_4C_1 \cdot |D_3E_5| , \\ &= \{ A_5D_3E_2 \cdot |a_2d_3e_5| - B_4C_1D_3 \cdot |b_1c_3d_4| + A_2B_4C_1 \cdot |a_1b_2c_4 \} . \Delta . \end{aligned}$$

3. Of course a different series of interchanges of indices may be taken to effect the transformation of $A_5B_1C_4D_3E_2$ into $A_2B_4C_1D_5E_3$; and, if so, a different form of the cofactor of $\Delta$ will be obtained. For example, we may interchange the indices of A and E, the indices of B and C, and then the indices of D and E, the intermediate terms thus being

$$A_2B_1C_4D_3E_5\,,$$
$$A_2B_4C_1D_3E_5\,.$$

This will give us

$$\begin{aligned} A_5B_1C_4D_3E_2 - A_2B_4C_1D_5E_3 = \;& A_5B_1C_4D_3E_2 - A_2B_1C_4D_3E_5 \\ &+ A_2B_1C_4D_3E_5 - A_2B_4C_1D_3E_5 \\ &+ A_2B_4C_1D_3E_5 - A_2B_4C_1D_5E_3\,, \\ = \;& -B_1C_4D_3\cdot|A_2E_5| + A_2D_3E_5\cdot|B_1C_4| + A_2B_4C_1\cdot|D_3E_5|\,, \\ = \;& \{-B_1C_4D_3\cdot|b_1c_3d_4| + A_2D_3E_5\cdot|a_2d_3e_5| + A_2B_4C_1\cdot|a_1b_2c_4|\}\cdot\Delta \end{aligned}$$

The two results are not at variance, for the difference between them is

$$-|a_2d_3e_5|\cdot D_3\cdot|A_2E_5| + |b_1c_3d_4|\cdot D_3\cdot|B_1C_4|\,,$$

*i.e.*, $D_3\cdot\{-|a_2d_3e_5|\cdot|b_1c_3d_4|\cdot\Delta + |b_1c_3d_4|\cdot|a_2d_3e_5|\cdot\Delta\}$,

*i.e.*, $0$.

4. To obtain the result in any given case it is not necessary to go through the whole process of proof. The cofactor may be written down with ease as soon as the so-called "intermediate terms" have been ascertained. Thus, if the cofactor of $\Delta$ in $A_1B_2C_3D_4E_5 - A_5B_4C_2D_3E_1$ be wanted, we write down the given terms with the "intermediate terms" placed in order between them—viz.,

$$A_1B_2C_3D_4E_5\,,$$
$$A_5B_2C_3D_4E_1\,,$$
$$A_5B_4C_3D_2E_1\,,$$
$$A_5B_4C_2D_3E_1\,,$$

and ask ourselves what factor is common to every consecutive two. The answer being $B_2C_3D_4$, $A_5C_3E_1$, $A_5B_4E_1$, the required cofactor is seen to be

$$B_2C_3D_4\cdot|b_2c_3d_4| + A_5C_3E_1\cdot|a_1c_3e_5| - A_5B_4E_1\cdot|a_1b_4e_5|\,,$$

the minus sign in the last term being due to the fact that the last of the three common factors occurs first along with $C_3D_2$ instead of $C_2D_3$.

5. In the case where the original determinant is of the 3rd order, the cofactor takes a peculiar form which is worth noting. For example, by the theorem we have

$$A_3B_2C_1 - A_2B_1C_3 = \{ - C_1c_1 - A_2a_2\} \cdot \Delta .$$

But

$$\begin{aligned} - C_1c_1 - A_2a_2 &= - c_1 |a_2b_3| - a_2 |b_1c_3| , \\ &= - c_1a_2b_3 + c_1a_3b_2 - a_2b_1c_3 + a_2b_3c_1 , \\ &= a_3b_2c_1 - a_2b_1c_3 . \end{aligned}$$

And so in other cases: Consequently, *in the case of determinants of the 3rd order, the difference of any two terms of the adjugate is divisible by the original determinant, the quotient being the difference of the corresponding terms of the original determinant.*

6. This special theorem is easily seen to be its own Complementary. There is thus suggested a different mode of proving the general theorem—viz., by means of the Law of Complementaries.*

Taking the case of the difference $A_5B_1C_4D_3E_2 - A_2B_4C_1D_5E_3$ above dealt with, we begin with the corresponding difference in the original determinant, and proceed as follows:—

$$\begin{aligned} a_5b_1c_4d_3e_2 - a_2b_4c_1d_5e_3 = {} & a_5b_1c_4d_3e_2 - a_2b_1c_4d_3e_5 \\ & + a_2b_1c_4d_3e_5 - a_2b_4c_1d_3e_5 \\ & + a_2b_4c_1d_3e_5 - a_2b_4c_1d_5e_3 , \\ = {} & - b_1c_4d_3 \cdot | a_2e_5 | + a_2d_3e_5 \cdot | b_1c_4 | + a_2b_4c_1 \cdot | d_3e_5 | ; \end{aligned}$$

whence by the Law of Complementaries

$$A_5B_1C_4D_3E_2 - A_2B_4C_1D_5E_3 = \Delta \cdot \{ - B_1C_4D_3 \cdot | b_1c_3d_4 | + A_2D_3E_5 \cdot | a_2d_3e_5 | + A_2B_4C_1 \cdot | a_1b_2c_4 | \} ,$$

as was to be shown.

7. The theorem applies, however, not only to the adjugate itself, but to any *minor* of the adjugate; that is to say, *the difference of any two terms of any minor of the adjugate determinant is a multiple of the original determinant.*

* *Trans. Roy. Soc. Edin.*, xxx. pp. 1–4.

For if to each of the two terms in question, $T_1$, $T_2$ say, there be prefixed as a factor one and the same term S of the complementary minor, we obtain two terms of the full adjugate, the difference of which, $ST_1 - ST_2$, not only contains the original determinant $\Delta$ as a factor, but, as the proof of this in § 2 shows, contains S as well, S in fact remaining unaltered throughout the various steps of the proof. Consequently $T_1 - T_2$ is a multiple of $\Delta$.

8. Of course this latter theorem does not really depend upon the former. A more natural mode of procedure perhaps would have been to enunciate them from the first as one theorem, beginning with the minor of the 2nd order of the adjugate and proceeding upwards to the full adjugate.

It is, however, interesting to note that the case for the full adjugate of one order leads to that for certain minors of adjugates of higher orders. Thus for the full adjugate of the 3rd order we have

$$a_1c_2 |\cdot| a_2b_3 |\cdot| b_1c_3| - |a_1c_3|\cdot| a_1b_2 |\cdot| b_2c_3| = |a_1b_2c_3|\cdot\{-a_1|b_2c_3| - b_3|a_1c_2$$

and from this, by the Law of Extensionals, the additional letter and suffix $d_4$ being taken, we derive

$$|a_1c_2d_4|\cdot|a_2b_3d_4|\cdot|b_1c_3d_4| - |a_1c_3d_4|\cdot|a_1b_2d_4|\cdot|b_2c_3d_4|$$
$$= |a_1b_2c_3d_4| \times \{-|a_1d_4|\cdot|b_2c_3d_4| - |b_3d_4|\cdot|a_1c_2d_4|\},$$

which is a particular case of the theorem for a minor of the adjugate of the 4th order.

Similarly, by taking the additional letter and suffix $e_5$, we have

$$|a_1c_2d_4e_5|\cdot|a_2b_3d_4e_5|\cdot|b_1c_3d_4e_5| - |a_1c_3d_4e_5|\cdot|a_1b_2d_4e_5|\cdot|b_2c_3d_4c_5|$$
$$= |a_1b_2c_3d_4e_5|\cdot\{-|a_1d_4e_5|\cdot|b_2c_3d_4e_5| - |b_3d_4e_5|\cdot|a_1c_2d_4e_5|\},$$

which is a particular case of the theorem for a lower-ordered minor of the adjugate of the 5th order; and of course the series may be continued indefinitely.

## On the Measurement of Simple Reaction Time for Sight, Hearing, and Touch. By Prof. Rutherford, M.D., F.R.SS. Lond. and Edin.

(Read July 10, 1894.)

(*Abstract.*)

Sensori-motor reaction time is the interval that elapses between the stimulation of a sense organ and a motor response. The physiological process involved consists of (*a*) an afferent factor,—the stimulation of a sensory terminal, and transmission of an impulse along sensory nerve fibres to the brain; (*b*) a psychical factor, involving an act of sensory perception and the voluntary production of a motor impulse; (*c*) an efferent factor,—the transmission of an impulse along motor nerve fibres, and consequent contraction of muscle.

To render the reaction simple, *discrimination* is eliminated from the act of perception by repeating the same sensation again and again without altering its character; and *choice* is eliminated from the voluntary act by giving the same motor response again and again. In the author's experiments the motor response was given in the usual way by the right forefinger closing an electrical key. The stimulus for sight was the movement of a flag attached to a lever; that for hearing was a click, given by transmitting an induction shock through a telephone; that for touch was an induction shock, or a mechanical tap. The moments of stimulation and response were recorded on a cylinder, and also on a pendulum myograph, and the time interval was measured with a chronograph and tuning-fork in the usual way. The pendulum myograph, although not hitherto employed in such experiments, is very advantageous in experiments on hearing and touch. Successive curves are superimposed, so that variations in the time of successive reactions are visible at a glance, and can be readily compared and measured. The record can be readily photographed and thrown on a screen for demonstration.

The reaction times, as measured by the author's methods, differ considerably from those of some German observers. In observations made on eight intelligent healthy men, varying in age from nineteen to sixty-two, the reaction time for sight varied from 0·1662 second to 0·2202 second, and was mostly between 0·20 and 0·22 second.

The reaction time for hearing varied from 0·1448 second to 0·1930 second, and was mostly between 0·15 second and 0·16 second.

The reaction time for touch varied from 0·1416 second to 0·1906 second in the different cases. The shortest time is that following stimulation of the cheek: it varied from 0·141 second to 0·157 second. When the skin of a finger was stimulated the reaction time varied from 0·142 second to 0·190 second, but was mostly from 0·15 second to 0·18 second in the different cases. There was no evident relation between the age of the individual and the length of his reaction time. In a limb the reaction time is generally longer the greater the length of sensory nerve traversed by the impulse; but there may be considerable variations in the reaction times for different districts in the field of touch that are not explicable by difference in the length of sensory nerve traversed, but are probably due to difference in the closeness of relation between centres for tactile sense in the brain and the motor centre for the hand. It may therefore happen that a response is given sooner by the hand when its skin is stimulated, than when the mucous membrane of the mouth is stimulated, although in the latter case the impulse has a much shorter tract of sensory nerve to traverse.

When the response is given by the *right* hand, the shortest reaction times for hearing and touch are obtained by stimulating the *right* ear and *right* cheek, so that the sensory impulse may be transmitted directly to the left side of the cerebrum, from which the motor impulse for the right hand emanates. In the experiments on sight, both eyes were used at same time. The influence of fatigue on the reaction time for hearing and the remarkable restorative effect of tea were demonstrated in photographs thrown on the screen.

## An Experiment on the Influence of Thyroid Feeding on the Proteid Metabolism in Man. By J. J. Douglas, M.B., C.M., F.R.C.P. Edin. (*From the Research Laboratory of the Royal College of Physicians of Edinburgh.*)

(Read January 7, 1895.)

The peculiar train of symptoms which have been found to follow removal of the thyroid gland in animals, and the connection of myxœdema with changes in the thyroid, have been known for a number of years.

The discovery that the onset of the symptoms may be prevented by the grafting of pieces of thyroid, and that myxœdema may be cured by the internal administration of the gland substance, clearly indicates that the thyroid gland forms *something* which exercises an important influence on the metabolic changes in the body. A study of the symptoms after removal of the gland and of myxœdema would lead to the belief that in both the ordinary rate of metabolism is diminished. Does the administration of thyroid act by stimulating the diminished metabolism? The remarkable loss of weight which accompanies thyroid feeding suggests that such is the case. So far, however, only one observation appears to have been made upon the subject. Ord and White published (*Brit. Med. Jour.*, 1893, p. 217) the results of their observations on a case of myxœdema treated with thyroid feeding. The nitrogen of the ingesta and the total nitrogen of the urine, as well as the urea, were daily estimated. Whether the nitrogen of the ingesta was directly estimated each day, or whether it was simply calculated from diet tables, does not appear. Nor are the methods of estimating the nitrogen and urea described. The observations extended over a period of six days during which no thyroid was given, and over thirty-three days during which thyroid was administered. The patient lost weight. The nitrogen of the urine increased in amount, and enormously exceeded the nitrogen of the ingesta. The metabolism of the proteids was, in fact, markedly increased.

Such an observation shows the influence of thyroid feeding in stimulating the proteid metabolism in *myxœdema*. But has it the same effect in health? To elucidate this question the following experiment was performed.

*Experiment.*

The subject was a patient in the Edinburgh Royal Infirmary, placed at my disposal through the kindness of Dr Byrom Bramwell. Kate M'G——, aged 22, a domestic servant, suffering from lupus of the face, but otherwise healthy. She was not confined to bed, but was not allowed out of the ward. She was instructed to keep carefully all urine and fæces, and to eat the whole of the diet provided and nothing else, and in all these respects she was most exemplary.

On the first day of the experiment the patient was given unlimited quantities of the food intended for her fixed diet, and told to eat as much as seemed to satisfy her. This was found to be as follows:—

| | | |
|---|---|---|
| Oatmeal, | 80 | grms. |
| Wheaten Biscuits, | 150 | „ |
| Butter, | 20 | „ |
| Sugar, | 40 | „ |
| Rice, | 75 | „ |
| Mince Collops (cooked), | 100 | „ |
| Liebig's Extract, | 12 | „ |
| Cheese, | 40 | „ |
| Milk (condensed), | 80 | c.c. |
| Tea Tabloids, | 6 | „ |

This diet was adhered to throughout the experiment. All the various articles were weighed in the laboratory, and sent to the hospital each day. The oatmeal and rice were weighed raw, and cooked in the ward. At the beginning of the experiment a large quantity of mince collops was cooked and sterilized in a large flask, and the quantity required weighed out daily.

The nitrogen of this diet was determined by Kjeldahl's method, and was found to be as follows:—

| | | | | | |
|---|---|---|---|---|---|
| Oatmeal, | 2·70 | per cent. | Total = | 2·16 | grms. |
| Biscuits, | 1·92 | „ | „ | 2·91 | „ |
| Rice, | 1·05 | „ | „ | ·78 | „ |
| Mince Collops, | 5·31 | „ | „ | 5·31 | „ |
| Liebig's Extract, | 8·5 | „ | „ | 1·02 | „ |
| Cheese, | 5·75 | „ | „ | 2·30 | „ |
| Milk, | 1·9 | „ | „ | 1·52 | „ |
| | | | | 16·00 | „ N. |

The quantity of the fluid was not limited.

The diet agreed with the patient. The pulse and temperature remained normal throughout. For the first three days she was kept on the diet alone; for the next five she had thyroid tabloids (Burroughs & Welcome's) in increasing doses, and for the last two days of the experiment the thyroid extract was stopped. It had been intended to carry on the experiment a little longer, but the patient began to menstruate, which made it impossible. That menstruation has no disturbing influence on the metabolism has been shown by recent work done under von Noorden's direction. The patient was weighed periodically. All the nitrogen estimations were done by Kjeldahl's method. The fæces were first mixed with dilute $H_2SO_4$, and dried at 100° C.

*Influence on Metabolism.*

| Date. | Weight in Kilos. | Nitrogen of Food in grms. | Urine. | | | Fæces. | | Total Nitrogen excreted in grms. | |
|---|---|---|---|---|---|---|---|---|---|
| | | | Quantity in c.cms. | Sp. Gr. | Nitrogen in grms. | Weight (dried) in grms. | Nitrogen in grms. | | |
| 11.10.94 | 53·7 | 16·00 | 750 | 1025 | 11·277 | 0 | 0 | ... | ... |
| 12. | ... | ,, | 825 | 1027 | 12·284 | 24 | 2·194 | 14·478 | ... |
| 13. | ... | ,, | 1220 | 1021 | 12·810 | 15 | 1·233 | 13·043 | ... |
| 14. | 53·7 | ,, | 1736 | 1015 | 14·001 | 33 | 2·795 | 16·796 | $\frac{3}{16}$ of a gland. |
| 15. | ... | ,, | 1900 | 1017 | 15·051 | 23 | 1·403 | 16·454 | ,, |
| 16. | ... | ,, | 2200 | 1013 | 15·510 | 26 | 2·089 | 17·589 | $\frac{6}{16}$ of a gland. |
| 17. | ... | ,, | 1680 | 1018 | Lost | 46 | 3·135 | ... | ,, |
| 18. | 51·4 | ,, | 1500 | 1022 | 15·490 | 23 | 1·132 | 16·622 | 1 gland. |
| 19. | ... | ,, | 1400 | 1021 | 16·100 | 28 | 1·799 | 17·899 | ... |
| 20. | 51·0 | ,, | 1120 | 1024 | 15·800 | 33 | 1·841 | 17·641 | ... |

During the experiment 16 grms. of nitrogen were taken per diem. Before the administration of thyroid 1·71 grms. were, on the average, daily excreted in the fæces. Thus 14·29 grms. were absorbed. During these two days an average of 12·5 grms. were excreted in the urine, leaving a daily gain of 1·79 grms. of nitrogen, or 11·2 grms. proteid, *i.e.*, 53·7 grms. of flesh.

During and after the administration of thyroid 2·08 grms. of nitrogen were daily excreted in the fæces, so that 13·92 grms. were absorbed. The daily excretion in the urine was on an average 15·3 grms., so that there was a daily loss of 1·4 grms. of nitrogen from the tissues, *i.e.*, of 8·75 grms. of proteid, or a daily loss of 42·0 grms. of flesh, or 294 grms. during the period.

In these calculations—1 grm. N. = 6·25 grms. Proteid = 30 grms. Flesh (*Munk Ueber die Ernährung*, p. 13).

But during this period the patient lost between 3000 and 4000 grms., and it would appear, therefore, that the metabolism of the non-nitrogenous constituents was very greatly increased. An examination of the excretion of carbon dioxide during thyroid feeding is highly desirable.

### *Absorption of Proteids during Thyroid Feeding.*

The average excretion of nitrogen in the fæces was—

| | | | | |
|---|---|---|---|---|
| Before feeding | 1·713 | grms. | = 10·7 | grms. proteid. |
| During „ | 2·108 | „ | = 13·1 | „ „ |
| After „ | 1·820 | „ | = 11·3 | „ „ |

It would thus appear that thyroid feeding interferes with the absorption of the proteids of the food.

### *Conclusions.*

The experiment clearly indicates—

1*st*. That thyroid feeding markedly increases the proteid metabolism in health as well as in myxœdema.

2*nd*. That its action is immediate, and continues for at least two days after it has been stopped.

3*rd*. That the loss of weight is probably more due to waste of non-nitrogenous tissues as fat than to waste of nitrogenous tissues.

4*th*. That the absorption of proteids is diminished while the thyroid is being taken.

I have to thank the superintendent of the laboratory, Dr Nöel Paton, for assistance and advice in carrying out this observation.

## Further Note on the Volume Changes which accompany Magnetisation in Iron and Nickel Tubes. By Professor C. G. Knott, D.Sc., F.R.S.E., and A. Shand, Esq.

(Read February 18, 1895.)

In our last communication (read July 2, 1894) we gave the broad results for three nickel tubes of various bores (see vol. xx. p. 296). In these experiments the screw stopper through which the capillary glass tube passed was made of brass. This brass stopper was used because of the comparative difficulty of working nickel. We were led, in the course of some tentative experiments, to use this brass stopper with one of the steel tubes. Now, in the earlier experiments with the iron and steel tubes, we had used an iron stopper. We were hardly prepared, however, to find that the mere substitution of a brass for an iron stopper should produce such a marked effect, not only on the amount of change of volume in given magnetic fields, but also on the manner in which the volume change varied as the field increased or decreased steadily.

This result at once proved that it was absolutely necessary to furnish the nickel tubes with a nickel stopper; and, in the present note, we give a comparison of the volume changes in Steel Tube V. with (1) an iron stopper, (2) a brass stopper, and of the volume changes in Nickel Tube I. with (1) a nickel stopper, (2) a brass stopper. The numbers in the first column give the magnetic fields, the others represent dilatations or changes per unit volume, the unit being $10^{-7}$ cubic centimetre.

| Field. | Steel Tube V. | | Nickel Tube I. | |
|---|---|---|---|---|
| | Iron Cap. | Brass Cap. | Nickel Cap. | Brass Cap. |
| 560 | −58·7 | − 0·6 | −110 | −107 |
| 500 | −42 | − 9·6 | −109 | −106 |
| 400 | −30 | −18 | −107 | − 98 |
| 300 | −18 | −12 | − 96 | − 89 |
| 200 | 0 | − 7·2 | − 72 | − 67 |
| 100 | + 7·2 | − 2·4 | − 29 | − 23 |
| 50 | + 3·0 | − 1·2 | − 4·5 | − 4·5 |

In the case of the nickel tube, the difference in the two columns is not of any great moment. With the nickel cap or stopper the changes of volume are slightly greater. This is very much what one would naturally expect to find. For the nickel stopper has the effect of lengthening by an inch or so the magnetisable bar, and the tube portion of the bar so lengthened will be more effectively magnetised than when the stopper is of a non-magnetic metal.

But it will be noticed that the behaviour of the steel tube is greatly altered when the brass cap is substituted for the iron cap. Thus there is no change of sign in field 200 when the brass cap is used, and there is no maximum negative dilatation in field 400 when the iron cap is used. In short, the character of the stopper has a marked effect upon the whole character of the strain. It is easy to see that the iron stopper will tend to increase the induction into the tube ; but, except in the way of diminishing the induction, and so altering the accompanying change of length, we cannot credit the brass stopper with any effect upon the longitudinal strain. It is as a disturbing factor in the transverse strain that we must consider it. It does seem extraordinary, however, that a stopper, whose hampering action is confined to one end of a fairly long tube, should, by its inability to yield to the magnetising forces acting on it, produce such a marked effect upon the volume changes taking place in the tube as a whole. A closer investigation of the phenomenon may throw some light on the character of the strain accompanying magnetisation.

## Notes on a Peculiarity in the Form of the Mammalian Tooth. By J. Smith, M.D. (Illustrated.)

(Read December 3, 1894.)

The ideal type of a mammalian tooth is here assumed to be a modified cone, or, as in certain molar teeth, a combination of cones, the vertex or vertices, as the case may be, corresponding either to the apex of the fang, or to the free extremity of the crown, or to both, as in the typical canine tooth, where two cones seem united by their bases. (Fig. 4.)

Such cone is supposed to be one of considerable altitude, with the axis at right angles to the base. In the natural tooth, however, the axis would, under various circumstances, be more or less bent, or arched, or oblique in its direction, and the cone itself laterally compressed or flattened, or in various other ways modified. (Figs. 1, 2, 3.)

In the morphology of the joints and certain other parts of the skeleton the principle of a spiral or curve of double curvature has been traced by the researches of Meyer, Langer, Henke, Meissner, Goodsir, and others; and so far back as 1858, a short paper by me, "On the Condyle of the Human Lower Jaw," and, in accordance with such views, was communicated to this Society by Professor Goodsir. Further inquiries led me, in 1864, to advance the proposition that a spiral tendency was indicated in the form of the human tooth, although in its case, owing to various causes, this confirmation is in many instances comparatively indistinct. Since that time, again, a number of observations made on the teeth of various orders of mammals have suggested some additional views on this subject, as not only interesting in a morphological and teleological aspect, but possibly of some surgical importance.

Speaking generally, then, the single-fanged mammalian tooth, as represented by such elongated and compressed cone, seems to be again modified by being twisted upon itself. This ranges from the faintest trace of a spiral indicated in the form of the tooth, as if it were slightly rotated on its long axis, to the development of what resembles a complete screw thread winding round its whole length as a central pillar or columella, the thread of such screw increasing in breadth and thickness with the diameter of the conical surface round which it is developed.

The Monodon or Narwhal (fig. 5) affords, perhaps, the best marked example of a spiral tooth with which naturalists are acquainted. In the tusk of this animal the whorls forming the spire are very complete throughout its whole length, which sometimes extends to 10 or 12 feet. In perfect specimens each whorl is separated from the other by a well-marked suture, forming a screw of considerable pitch, and winding round the tooth in such a manner as to render its appearance similar to that of some of the tapering spiral shells of the Gasteropods, such as the Turritelidæ. These teeth in the Narwhal have been erroneously described, even by such authorities as Cuvier, Owen, C. S. Tomes, and others, as incisors. Professor Sir William Turner, however, has shown them to be unmistakably canines, and belonging entirely to the true maxillary bone of each side. In their case a curious departure takes place from that symmetry of form normally exhibited in corresponding structures on the two sides of the body. Of these two canines there is rarely more than one developed, and that is the Left one, the Right being usually suppressed. In some instances both teeth are present, but there appears to be no instance of the Right tooth being developed, and the Left suppressed. In this left tooth the spiral winds from right to left, constituting what is familiarly known as a left-hand screw ; occasional examples, however, have been met with where, in the adult animal, both teeth are developed, and what seems remarkable in these cases is, that the direction of the screw—that is, from right to left—is found to be the same in each tooth ; while this arrangement is still more widely established as the general rule, by finding in the fœtal Narwhal, of which more numerous specimens are at our command, that the spiral markings, although less distinct, are of the same character in the two teeth, while neither one nor other of them have been as yet erupted. Curiously enough, the direction of the screw characteristic of these teeth corresponds to the principle of construction prevailing in the mammalian dentition.

Another group of the cetacea, known as the Ziphiods, affords an almost equally well-marked although somewhat different form of spiral construction in the teeth of some of its species, as in the Mesoplodon Layardi described by Professor Turner in the "Challenger Reports." This animal presents some very remarkable features in the character of these organs: two dental structures exist on the lower jaw, one on each side, which curve upwards and outwards, then obliquely backwards, and finally turn inwards until they cross each other like a loose belt buckled above the upper jaw or snout, so that the lower jaw cannot be depressed, or the mouth opened, except to a very limited extent. The curve or spiral taken by these teeth is more difficult to describe, or even to recognise, than in the case of the Narwhal; but, unlike it, the spiral is symmetrical—that is, it takes a reverse direction on opposite sides of the head. By far the largest portion of the tooth is formed by the fang. This consists of a flat broad plate, somewhat crescentic or falciform in its outline, with the concave edge looking upwards and forwards—one extremity of the crescent being rooted in the lower jaw—from which this flat crescent-shaped plate rises with its inner surface in relation to the outside of the upper jaw, and is finally carried backwards, and bent or twisted across the dorsum of the head or snout (fig. 6). It is exclusively in the fang of the tooth that this peculiar contour is established, the crown proper taking no part in such flexure, since that portion of the tooth is represented merely by a small triangular protuberance situated upon the flat surface of the upper and extreme end of the fang. The somewhat complex curvature here described approaches what would result from winding a flat strip of flexible metal or other material very obliquely and for about one turn round a column of considerable diameter—such columnar axis being then withdrawn, leaving the strip to represent the thread of the screw thus formed.

The two examples here given may serve to typify the modes in which a spiral construction presents itself in the long axis of certain mammalian teeth—being in the one case illustrated by the thread of the screw-nail, and in the other more or less by the worm of the cork-screw. Along with such spiral configuration, however, which seems almost universal, it is impossible to overlook another peculiarity of form in many teeth, and with which it is indissolubly associated. This is the bend or arching throughout their whole length occurring in numerous well-known examples among these organs. This bend or arching, extending from the alveolar attachment to the crown extremity of the tooth, forms, in many animals, an arc equal to the greater part of a circle, or is even somewhat more extensive, a complete ring being occasionally constituted by the tooth, the tip or free extremity returning to a spot somewhere about the situation of the formative pulp. The general rule, however, is, that the arc thus formed is not that of a circle described upon one plane, or of a discoid character, but oblique in the course it follows, so that the free extremity, instead of impinging upon, passes on one or other side of the alveolar or maxillary end of the tooth, the primary curve again being at the same time of such a nature that, instead of a ring, it approaches in character more to a logarithmic spiral.

The upper canine or tusk of the Walrus affords an example where some of these morphological features are easily discernible (fig. 7). This tooth projects downwards at about a right angle with the jaw, and is so bent as to form an arc of a very large circle, having the convexity directed forwards and somewhat outwards. The body of the tooth consists of a very elongated and laterally compressed cone, so that a transverse section of it would exhibit an oval or elliptical form. The bend in these organs is too apparent to require any remark; but that a spiral turn is also given to them on the long axis, may, in addition to what is perceptible on mere ocular inspection, be demonstrated by looking at the two cut surfaces of any cross section, when the varying direction in the vertex diameter of the ellipse presented at either end will indicate the twist or turn on its long axis which the tooth is taking.

So far, again, as concerns the bend taken by the Walrus tusk, a much more pronounced arc is described by those of some other animals; a circumstance occasionally interfering with the axial torsion being so clearly followed out. The incisors of the Elephant, the canines of the Hippopotamus, the enormous tusks of the extinct Mammoth, &c., describe, in the arc presented by some of them, a large portion or even the whole of a circle, so deflected outwards, however, that instead of a true ring it forms one of those curves of double curvature already spoken of, thus enabling the free extremity to pass on one side of the animal's head. The long axis twist, however—making allowance for such flexure—is still quite traceable in all these examples, and is directed, as it generally seems to be throughout the mammalia, from the mesial side of the base towards the outer side of the free extremity of the tooth, or as if the exposed portion of the tooth had been rotated slightly backwards and inwards. In the Hippopotamus this configuration, owing to the trihedral form of the canines, admits of a different mode of demonstration (fig. 8). The lower canine—the most typical tooth in this animal—presents three surfaces, constituting a prism, with one of its faces directed posteriorly. On making a more careful examination of this face, it will be found that at the base of the tusk it looks partly outwards as well as backwards, while, as we approach its apex, the aspect of this posterior plane is gradually altered till it looks exclusively backwards, thus showing the tooth to have been, as it were, rotated backwards and inwards at its extremity for about a quarter of a turn on its long axis.

In the Suidæ, again, both the axial rotation and the bending of the tooth upon itself are well represented. Taking the canines of the Phacocærus or Wart-Hog as a typical example, they constitute horn-like organs projecting from the sides of the mouth, those from the lower jaw curving outwards and upwards, while the formidable tusks of the upper jaw curve outwards,—forwards and then upwards also,—so that when the mouth is closed the anterior convexity of the upper is in relation with the posterior concavity of the lower tooth (fig. 9). Greater complexity in regard to the direction of the long axis spiral is here occasioned not only by this remarkable corniform bend or arching of the tusk, but by the upper tusk pointing upwards while properly it ought to point downwards instead. While this upward curvature of the upper canine is unusually conspicuous in the Wart-Hog, the same peculiarity attains its maximum character in the Babyroussa Hog, where the upper canines assume this upward bend, not after issuing from the mouth, but where they seem actually to emerge from the upper aspect of the superior maxilla, piercing the integuments above them, and protruding through the skin at each side of the snout (fig. 10). In this manner they present the appearance of two long and slender horns arching upwards and backwards, each describing more than a semicircle, the extremity of which just clears the side of the animal's head. An apparent discrepancy in the development of the upper canine of the Babyroussa in this way presents itself. The tooth seems to spring from the upper instead of the lower or oral border of the superior maxilla, and would thus suggest an inversion of its formative structure; but in other members of the Suidæ it has been shown that while the upper canine, at an early stage of its development, necessarily points downwards, the tooth, as it increases in length, curves upon itself, and, departing from the primary direction in the original axis of its alveolus, describes in its growth a circular sweep till its free extremity at last points upwards. And if it be kept in mind, along with this, that after the eruption of a tooth the alveolar walls continue to extend and adapt themselves alongside the lengthening fang, the difficulty is so far met, since the dental germ may have followed the usual course of development, while the subsequent mode of growth, accompanied by an altered form and direction of the alveolus, has produced in the Babyroussa what is possibly no more than an exaggerated example of similar peculiarities seen in other members of this family. There is another circumstance connected with such curved teeth which also renders the axial twist less certainly determinable, and that is its being in their case frequently ill-marked and often abnormal, in so much that the tusk, instead of passing to the outer side of the head, occasionally inclines inwards and infringes upon or actually perforates the skull. The normal direction of the spiral, however, may be realised in a well-formed specimen of any of these exceptional organs by supposing the tooth to be restored to the condition of a straight spire, and, in such as those of the Babyroussa, the alveolus and contained upper tusk to be completely inverted, when the spiral turn will be perceived to be the same as in the case of other animals, where the crown or exposed portion of the tooth appears as if it were rotated on its long axis somewhat backwards and inwards.

In the Rodentia, the incisors are illustrative of the same peculiarities in form and construction. The combined free-cutting portion and the imbedded fang of these organs constitute a very much-prolonged tooth where the axial torsion is readily perceived, especially in the larger animals belonging to this order, such as the Capybara, the Porcupine, or the Paca, in which latter animal the twist is very apparent, the spiral winding in the same direction as in the other examples given, namely, as if the tooth were rotated on its long axis slightly backwards and inwards at its free extremity. Besides exhibiting this spiral rotation, these teeth are generally also very much bent or arched, the arch in some cases measuring fully half a circle. Neither this arching, however, nor certain curvatures necessarily resulting from following the contour of the jaw in which they are for so long a distance deeply implanted, interfere with the axial spiral under notice being easily traced.

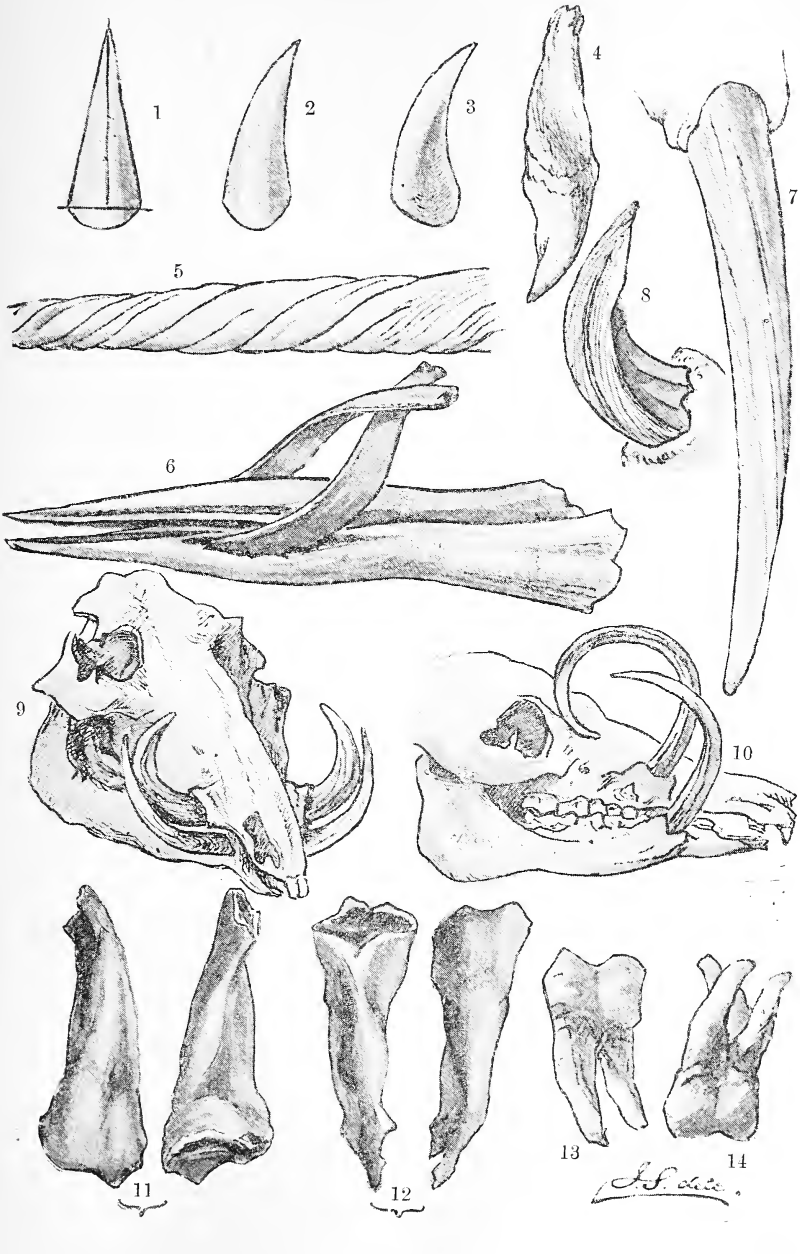
The foregoing examples are selected from among teeth of persistent growth, or where the pulp-structures continue the development of the tooth during the animal's lifetime. In such teeth, however, as those where growth and development cease with the completion of the fang or root, the same principles are exhibited in their conformation. Many of these teeth are bent or arched like some of those previously mentioned. In those which best display this formation, the crown and fang appear like two cones united by their bases, such as the canines of the Carnivora (fig. 4). Viewed in profile, they commonly present a crescentic form, extending from the free extremity of the crown to the termination of the root; the arc thus formed, however, being in all of them comparatively inconsiderable. With regard to the spiral in their long axis it is different; this character is quite apparent in all, and in many of them is equally well-marked, as it is in those already specified, and the line of torsion is in the same direction—namely, from the mesial side of the fang diagonally across the front of the tooth to the distal side of the crown. Among teeth of this character some fair examples are presented by the Seals. The simplest type of tooth in these animals is that where it consists of two cones united by their bases—the smaller cone forming the crown, the larger and somewhat ovate-shaped one forming the fang. The whole tooth is bent till it approaches an arc of about a quarter of a circle; and if such a tooth be imagined to be grasped at the crown by the finger and thumb of one hand, and at the fang end by the finger and thumb of the other, and the two extremities be supposed to be then rotated for a quarter of a turn in opposite directions, it will enable the configuration of the tooth to be realised. Much the same may be said of such teeth as the canine in the Lion, Tiger, Bear, &c., where very similar characters are presented, both in the bend and axial rotation, as well as in the external form of the tooth. The incisors of the Horse afford another obvious and well-defined example of the same peculiarities (figs. 11 and 12). These teeth, crown and fang conjointly, are bent backwards so as to form an arc having its convexity directed anteriorly. Seen from the front, the tooth appears as a cone of which the cutting edge represents the base and the apex of the fang the vertex, such cone being twisted on its long axis in a similar manner to the other teeth already noticed. Upon closer inspection, however, the appearance of the fang seems to indicate something more than mere axial rotation, and rather suggests the winding round it of what would be equivalent to a double screw thread. Taking an upper incisor as an example, one such thread is traceable in a somewhat salient track winding very obliquely round the front of the tooth from the distal side of the crown towards the mesial side of the fang; and, posteriorly, another similar thread is found, now, however, winding from the mesial side of the crown to the distal side of the fang: thus constituting a double-threaded screw combination developed on the conical body of the root. In using the term "screw thread," it must, of course, be understood as employed not with geometrical exactitude, but merely as indicative of an oblique form of surface development imparting to what would otherwise be merely a simple cone or wedge a certain amount of a halicoid conformation, probably giving additional steadiness to the fang, and altering the nature and direction of those forces acting upon a tooth in the exercise of its functions.

Among the Primates, the large canines of the male Gorilla, the Orang, and the Mandrill, may serve to exemplify how the teeth in these animals accord with the description of the turn apparent in the crown and fang of others already mentioned. But, as we ascend in the scale of animal life, these characters seem to become less perceptibly marked until the human species is reached, when little more than vanishing traces of them seem to remain.

This comparatively feeble marking of the teeth in Man makes it more difficult to recognise in them so perspicuously the principle of formation found in the more typical illustrations adduced. And, in addition to this fading from the original model, the disadvantage in the human tooth is increased by malformations and irregularities in the form and development of the fang—there being like irregularity in their position more common in the teeth of the human subject than in those of the lower animals. At the same time, while they may not so manifestly exhibit the screw thread appearance, they sufficiently indicate an axial turn or twist in their construction. This in many cases becomes more easily perceptible on looking along the tooth on its long axis from the apex of the fang towards the crown, when the tendency to a slight spiral or winding in the contour will be at once apparent, the crown face of the tooth being turned inwards at its distal border, while the fang extremity inclines in an opposite direction.

A somewhat curious circumstance attaches to the formation of the multiple-fanged teeth—the molars—in the human subject. In a lower molar tooth, with its two fangs well developed (fig. 13)—the larger and better marked being anterior, and the smaller posterior—each will be found somewhat flattened as in the single-fanged teeth, and, like them, each one indicating the same bias towards the spiral tendency they present. Each fang in this way appears as if it belonged to a single-fanged tooth, but that two such teeth had been fused together at their crowns so as to constitute one double-fanged molar.* Both these fangs are in the lower molars besides bent so as to be directed somewhat backwards. In the upper molars, however, where each tooth possesses three instead of two fangs, of much the same form as those of the lower jaw, the direction taken by one of them seems to differ from the others (fig. 14). The three fangs are arranged so that two of them are situated one behind the other on the outer or buccal side of the tooth, while the third is placed in the inner or palatal side of these. The two external fangs resemble those of the lower molars in being directed backwards; but in by far the greater number of instances the internal or palatal fang—the larger of the three—points, in the normally-formed tooth, slightly forwards, while the spiral rotation on its long axis would at the same time appear, in contrast to the others, as if it were reversed. These appearances are not very easily described, but they correspond to what would occur if the original tooth pulp

* An hypothesis of this kind has been advanced in regard to the molars of the Elephant and some other animals—extending even to Man—by Röse, Kukenthal, Dybowski, Gervais, Gandry, and others, in contradistinction to those, such as Leche, who believe the molars to be simple teeth.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could be imagined to have consisted, at some period of its existence, of a row of three pulps arranged in juxtaposition one behind the other, and the series of three subsequently folded back upon itself so that one of them was brought to the inside or palatal side of the others, without interfering with its individual formative growth. Perhaps the shortness of the maxillary border might account for something of this nature actually occurring by way of affording more accommodation.

Any teleological explanation of such characters in the configuration of the dental organs here noticed is obscure. Accidental circumstances altogether extraneous to the tooth's development, properly so called, may in certain cases possibly exercise some influence in their production. The natural growth of the jaw—or, on the other hand, its defective or restricted growth—may also contribute a certain moulding influence during the later period of the tooth's development. The facts, however, remain the same. Perhaps the most important practical bearing exercised upon the wellbeing of the tooth, rests in the conversion of the fang from what would otherwise be a simple wedge or cone to something approaching a screw; thus obviating the effect of direct pressure or impact in the line of the tooth's long axis, driving it into the socket, with the result of deepening and widening it each time such force is exerted, and so loosening the implanted fang. It seems, then, unnecessary to adduce any greater number of illustrative examples of the conformation than has been here advanced as characteristic of the mammalian teeth. There are, without doubt, instances where such conformation is far from being obvious, and there are others where an undoubted departure from it occurs. But, as a general rule, that a typical construction of the kind under notice exists, would appear to be sufficiently evident. The remarks here offered are not pretended to be either exhaustive or confirmative of the question, but are rather intended to be of the nature of a retaining note which may be further developed at some future time.

## On the Absorption of Carbohydrates by the Intestinal Epithelium: An Experimental Inquiry into Pavy's Theory of the Action of the Epithelium on Carbohydrates. By D. Noel Paton, M.D., F.R.C.P.E., and G. Lovell Gulland, M.D., F.R.C.P.E. (*From the Research Laboratory of the Royal College of Physicians of Edinburgh.*)

(Read January 21, 1895.)

One of the most important conclusions arrived at by Pavy in his *Physiology of the Carbohydrates*, is that the intestinal epithelium acts as a barrier to the passage of carbohydrates into the circulation by converting them into fats (p. 248 *et seq.*).

Although the fact that the amount of sugar in the portal blood varies with the amount of carbohydrates taken would seem to indicate that the intestinal epithelium passes sugar on to the blood for the most part unchanged, the well-known observations on the conversion of carbohydrates to fats in the animal body suggest at least the possibility that such a change as that maintained by Pavy may take place.

The experimental data on which he bases his conclusions are, however, eminently unsatisfactory. Because he finds that, in rabbits fed upon oats, the intestinal epithelium contains fat globules, and the lacteals a milky fluid, he concludes that the fats are formed from the carbohydrates of the oats, although the oats used are said to have contained 5 per cent. of fats—about as much as is contained in moderately fat ox flesh. The presence of such an amount of fat entirely vitiates the conclusion that the fat seen in the epithelial cells is derived from the carbohydrates.

The following experiments seem very clearly to indicate that the fats seen in the epithelium in Pavy's experiments are derived from the fats of the oats and *not* from the carbohydrates.

In these experiments the following methods were employed:—

*Method of Examination.*

The intestines of the animals were examined, in the earlier experiments, in three different ways—by scraping, by cutting sections with the freezing microtome, and by sections cut in paraffin. In the first case, the intestine, at varying distances below the entrance of the pancreatic duct, was opened as soon as the animal had been killed by chloroform; the villi were scraped; the scrapings were spread out on cover-glasses, mounted in 2 per cent. osmic acid, left for several hours, and then examined in the same fluid, or in glycerine run in beneath the cover-glass.

In the second case, the portions of intestine taken were put into a large quantity of 2 per cent. osmic acid for twenty-four hours, to ensure a thorough blackening of all fat, and were then cut at once with the freezing microtome, and examined in glycerine.

In the third case, after fixing as above in osmic acid, the pieces of intestine were washed in water for twenty-four hours, then rapidly hardened in alcohol of increasing strength, cleared on xylol, left in the paraffin bath for twenty-four hours, cut in the usual way, and mounted in xylol balsam. Flemming has shown that xylol, of all the clearing agents, has least effect on fat which has been blackened by osmic acid. We took care to expose the specimens to absolute alcohol and xylol for the shortest time that was consistent with complete dehydration and clearing, and we found, after the first half-dozen experiments, that our specimens, prepared in this way, not only showed no signs of solution of fat having taken place, but that, from the more perfect clearing of the tissue, we could see fat in them in cases where it was not to be made out by the two first methods. The paraffin sections had also, of course, the advantage of being much clearer and more regular in thickness than those cut by freezing.

In the later experiments, therefore, we confined ourselves entirely to the last method. We were careful, in almost every case, to examine at least two pieces of intestine, one taken a few inches below the pancreatic duct, the other at a lower level, near the termination of the small intestine.

*Experiments.*

*Experiment 1 Oats deprived of Fats.*—110 grms. of oats were pounded in a mortar with ether, and left in a bottle with ether for several hours. The ether was decanted off, and the oats extracted in the same way six times. The ether extract weighed 3·3 grms.

Two rabbits were starved for two days. To one the oats, deprived of fat as above described were given, to the other 110 grms. of oats from the same sample, not so extracted. Both rabbits took the food well—the whole quantity being consumed in about twelve hours. Twelve hours after, the rabbits were killed, and a piece of intestine four inches below the opening of the pancreatic duct was taken from each, along with a piece near the ileo-cæcal valve. In the first rabbit no fat could be detected in any of the cells, but in the second, fat in small and scattered globules was found in the cells at the tops of many of the villi.

*Experiment II.*—Two rabbits which had been in the same cage, and were in good condition, were taken. A was fed on oats; B received rice, which, however, it did not take well. On the second day of the experiment it was fed on turnips, which it took greedily. They were kept on these diets for ten days, and both maintained their weight.

*Weight in Grms.*

| | 15/11/9. | 26/11/94. |
|---|---|---|
| A. Oats, . . . . | 1510 | 1512 |
| B. Turnips, . . . . | 1780 | 1735 |

Oats contain about 5 per cent. of fat—5·6 of the dry substance. Turnips contain about 0·2 per cent. of fat—1·5 of the dry substance. In oats, fats are to carbohydrates as 1 to 11. In turnips, fats are to carbohydrates as 1 to 47.

On the 26th the animals were killed, and the intestine examined as described above.

In A there was a very small amount of fat in very small globules, and entirely at the tips of the villi. In B no fat whatever could be detected.

But it is only by experiments upon animals in which the alimentary canal can be readily and completely emptied that satisfactory results on this subject can be obtained. For this purpose rats are eminently suitable. Even after a copious meal, in from twelve to twenty-four hours the stomach and small intestine are found empty, and absorption is completed, as is shown by the next Experiment.

*Experiment III.*—Five rats were starved for twenty-four hours and then one, A, was killed, and the others were fed on beef fat and water. The food was given at night, and was freely eaten. In the morning the residue was removed from the cages. After four hours, one of the rats, B, was killed; another, C, was killed after twenty hours; another, D, after twenty-five hours; and the last, E, after forty-eight hours.

From the middle of the small intestine a small piece was taken in each case, and treated as above described.

A showed no sign of fat.

B showed the absorption of enormous quantities of fat. The whole length of each villus, right down to the mouths of the Lieberkühnian glands, was involved in the absorption; but while at the tips of the villi the fat globules were so large and numerous that nothing could be seen of the epithelial cells, towards the bases they greatly diminished in size and number.

C, D, and E showed no signs of fat absorption.

Having thus shown that absorption from the intestine is in the rat completed within twenty-four hours, the following experiments were made:—

*Experiment IV.*—Four white rats were starved for twenty-four hours. In the evening—

A got 17 grms. of lean horse flesh.

B got 7 grms. of starch and sugar.

C got 11 grms. of ox fat.

D got no food, and was found dead next morning. The others were killed in the morning, after feeding for twelve hours.

On examining the intestine, fat absorption was going on actively in C. In A and B and D there was no sign of fat in the intestine or epithelium.

*Experiment V.*—Two white rats were starved for twelve hours, and then fed on hard-boiled white of egg, starch, and water. White of egg contains 0·3 per cent. of fat (see Munk, *Ueber die Ernährung*, p. 142).

The food was given in the evening, and the residue removed in the morning. A large quantity had been taken.

A was killed after three hours.

B was killed after seven hours.

No fat was found in the intestinal epithelium of either.

## *Conclusions.*

These experiments clearly show—

1st, That the fat found in the intestinal epithelium of rabbits fed on oats is derived from the fats of the oat.

2nd, That there is not evidence that carbohydrates are converted to fats by the intestinal epithelium in the course of absorption.

## Note on Normal Nystagmus. By Prof. Crum. Brown.

(Read February 4, 1895.)

When, with head fixed, we allow the eyes to wander over a fixed scene before us, we find that the eyes do not move continuously, but by jerks, so that what we see is really a series of separate pictures, each of which is at rest, the jerk from one position to the next being so rapid that we practically see nothing during the change of picture. That this is so can be shown if we have a bright light—say, an incandescent electric lamp—in the field of vision. After allowing the eyes to wander over the scene before us, we find, on closing the eyes, that we see a series of separate sharp secondary images of the bright light. Even when we make an effort to move the eyes continuously we find, by means of the secondary images, that we have not succeeded in doing so.*

If, instead of keeping the head absolutely still, we move it, we find that in this case also the lines of glance—that is, the lines along which we look during the intervals between the jerks—remain fixed in reference to fixed external objects. It is obvious that during the interval between two jerks the muscles of the eyeballs must act so as exactly to compensate for the movement of the head, and this whatever be the axis about which the head is moving. Even when this axis is the line of vision, the compensating movement takes place, as may be shown by a simple experiment. Select an object upon which the eye is to be kept rigorously fixed during the movement of the head. This object should be about 15° or 20° distant from the bright light. Now, keeping the eye fixed on the mark, incline the head towards one shoulder, thus rotating it about a fore-and-aft axis, and then shut the eyes. We

* There is a device, however, by means of which we can move our eyes continuously so as to see, not a series of fixed pictures, but one moving picture, and get, not a number of distinct and sharp after-images of the bright object, but a band composed like a continuous spectrum of an infinite number of images, each infinitely near its neighbours. We can obtain this result if we have a moving object in the field, and keep our eyes constantly fixed on it.

shall see distinct sharp after-images of the light, showing that the eyes were, during the glances, fixed relatively to external objects, and therefore rotating relatively to the head about a fore-and-aft axis at the same angular rate as the head relatively to external fixed objects and in the opposite sense. All these compensating movements of the eyeballs relatively to the head have the effect of giving us a succession of fixed pictures of fixed things across which we see any really moving object move. We thus obtain that sense of the steadiness of the external world which is of great use to us in moving about in it.

These compensatory movements of the eyes are successful only when the angular movement of the head is not too rapid, for there is a maximum rate beyond which the eyeball cannot go: if the head moves faster than that, the eye does indeed make an effort to compensate and give us steady pictures, but these efforts are only partially successful. We can wag our head faster than the eyeballs can move; and when we do so, we see the world wag in the opposite direction; but the really fixed external objects which we see wagging do not seem to describe nearly so large an angle as the head actually does: the movements of the eyes compensate to a certain extent, though not completely, the movement of the head.

When we look for the cause of the phenomena we have been describing, we encounter a difficulty which is of the same kind as that which meets us in almost every question concerning our senses. We have been accustomed from our earliest infancy to use our senses, and our power of moving our eyes, our head, and our hands, so as to obtain, in ordinary cases, a consistent notion of what goes on around us; and we cannot usually tell how much of our information has come to us through one channel, and how much through another; but I think I shall be able to show that there are at least two causes of the normal nystagmus I have been describing. Helmholtz, who has described most of these phenomena, and has shown how they satisfactorily explain some striking and interesting optical illusions, refers them exclusively to the effort to fix external objects. We do not see anything very well unless we look at it, and to look at it we must keep our eyes fixed on it for an appreciable, though it may be a very short, time. This effort of fixation satisfactorily explains the jerkings of the eyes when the head is fixed; and if this cause is efficient when the head is fixed, it must also be operative when the head is moving.

But there are cases in which the compensating movement of the eyes cannot be explained in this way. These movements take place when the head rotates even when the eyes are shut, as was, I believe, first noticed by Dr Breuer, whose observations have been confirmed by every one who has taken the trouble to repeat his experiment. The whole facts clearly enough point to a reflex action, the sensory organ being the semicircular canals of the internal ear. It has often been objected to the kinetic theory of the action of the semicircular canals, that it is only under special experimental conditions that we are aware of the sensations of rotation, and that it is absurd to suppose that such a sense should only now be discovered. But though *we* pay no attention, as a rule, to messages coming to our brain from the ampullæ, the motor centres of the muscles of our eyeballs are not so negligent, and we benefit here, as in many other cases, by reflex action, of which we may be nearly or quite unconscious.

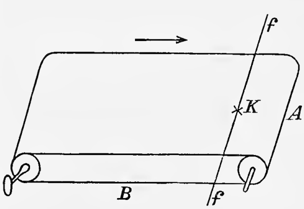
The conclusion to which the foregoing considerations seem to lead us is, that we see things fixed which are fixed in relation to our lines of glance—that is, the lines along which we look during the usually short intervals between jerks of the eye. This is all so clearly expressed by Prof. Mach in his *Beiträge zur Analyse der Empfindungen*, that one is surprised to find in the same book what seems a direct contradiction.

"Wir dachten uns bisher der Einfachheit wegen nur die fixirenden Augen bewegt, hingegen den Kopf (und überhaupt den Körper) ruhig. Drehen wir nun den Kopf ganz beliebig, ohne ein optisches Object absichtlich ins Auge zu fassen, so bleiben die Objecte hierbei ruhig. Zugleich kann aber ein anderer Beobachter bemerken, dass die Augen wie reibungslose träge Massen an den Drehbewegungen keinen Antheil nehmen. Noch auffallender wird der Vorgang, wenn man sich continuirlich längere Zeit um die Verticalaxe, von oben gesehen etwa im Sinne des Uhrzeigers, herumdreht. Die offenen oder geschlossenen Augen drehen sich dann, wie Breuer beobachtet hat, etwa zehnmal auf eine volle Umdrehung des Körpers gleichmässig verkehrt wie der Uhrzeiger, und ebenso oft ruckweise im Sinne des Uhrzeigers zurück." . . . "Niemand wird sich bei Wiederholung der Beobachtung der Ueberzeugung verschliessen können, dass man es mit einer durch die Körperdrehung reflectorisch ausgelösten automatischen (unbewussten) Augenbewegung zu thun hat. Wie diese Bewegung zu Stande kommt, bleibt natürlich zu untersuchen. Eine einfache Vorstellung wäre die, dass von zwei antagonistischen Innervationsorganen der ihnen bei der Körperdrehung gleichmässig zufliessende Reiz, von dem einen wieder mit einem gleichmässigen Innervationsstrom beantwortet wird, während das andere immer erst nach einer gewissen Zeit wie ein gefüllter und plötzlich umkippender Regenmesser einen Innervationsstoss abgibt. Für uns genügt es vorläufig zu wissen, dass diese automatische compensirende unbewusste Augenbewegung thatsächlich vorhanden ist.

"Die langsamere unbewusste compensirende Augenbewegung (die ruckweise hinterlässt keinen optischen Eindruck) ist also die Ursache, dass die Objecte bei Kopfdrehungen ihren Ort beizubehalten scheinen, was für die Orientirung sehr wichtig ist. Drehen wir nun mit dem Kopf in demselben Sinn, das fixirte Object wechselnd, auch *willkürlich* die Augen, so müssen wir durch die willkürliche Innervation die automatische unwillkürliche *übercompensiren.* Wir bedürfen derselben Innervation, als ob der *ganze* Drehungswinkel vom Auge allein zurückgelegt worden wäre. Hierdurch klärt es sich auf, warum, wenn wir uns umdrehen, der ganze optische Raum uns als *ein* Continuum und nicht als ein Aggregat von Gesichtsfeldern erscheint, und warum hierbei die optischen Objecte *festliegend* bleiben. Was wir beim Umdrehen von unserm eigenen Körper sehen, sehen wir aus klarliegenden Gründen optisch bewegt.

"So gelangen wir also zu der praktisch werthvollen Vorstellung unseres *bewegten* Körpers in einem *festliegenden Raume.*" (Pp. 59–61.)

"Wir stellen uns auf eine Brücke und betrachten das unter derselben durchfliessende Wasser. Dann empfinden wir gewöhnlich *uns* in Ruhe, das *Wasser* aber in Bewegung. Längeres Hinblicken auf das Wasser hat aber bekanntlich fast regelmässig zur Folge, dass plötzlich die Brücke mit dem Beobachter und der ganzen Umgebung dem Wasser entgegen in Bewegung zu gerathen scheint, während umgekehrt das Wasser den Anschein der Ruhe gewinnt. Die *relative* Bewegung der Objecte ist in beiden Fällen *dieselbe*, und es muss demnach einen *triftigen physiologischen* Grund haben, warum bald der eine, bald der andere Theil der Objecte bewegt empfunden wird. Um dies bequem untersuchen zu können, habe ich mir einen einfachen Apparat construirt, der in Figur 18 dargestellt ist. Ein einfach gemusterter Ledertuchlaufteppich wird horizontal über zwei 2 m lange, 3 m von einander in Lagern befestigte Walzen gezogen, und mit Hülfe einer Kurbel in gleichmässige Bewegung gesetzt. Quer über den Teppich, etwa 30 cm über demselben, ist ein Faden ff mit einem Knoten K gespannt, der dem bei A aufgestellten Beobachter als Ruhepunkt für das Auge dient. *Folgt* der Beobachter mit den Augen den *Zeichnungen* des im Sinne des Pfeiles bewegten Teppichs, so sieht er diesen in Bewegung, sich und die Umgebung aber ruhig. Fixirt er hingegen den *Knoten*, so glaubt er alsbald mit dem ganzen Zimmer dem Pfeile entgegen in Bewegung zu gerathen, während er den Teppich für stillstehend hält. Dieser Wechsel des Anblicks vollzieht sich je nach der Stimmung in längerer oder kürzerer Zeit, gewöhnlich nach einigen Secunden. Weiss man einmal, worauf es ankommt, so kann man ziemlich



rasch und willkürlich mit den beiden Eindrücken wechseln. Jedes Verfolgen des Teppichs bringt den Beobachter zum Stehen, jedes Fixiren von K oder Nichtbeachten des Teppichs, wobei dessen Zeichnungen verschwimmen, setzt den Beobachter in Bewegung." (Pp. 65, 66.)

My experience does not coincide with that of Prof. Mach, as given in the passage last quoted. When I look over the parapet of a bridge at the water flowing below, I see the water move and the bridge steady when my lines of glance are fixed relatively to the bridge, but I can fix points in the moving water, masses of froth, or eddies, or bits of stick; and when I do so, and jerk my eyes from one portion of the moving mass to another, then my lines of glance being fixed relatively to the water, I see the water at rest and the bridge moving. In the same way, on a moon-light night, when there are light clouds in the sky, we can see the moon moving and the clouds fixed, or the clouds moving and the moon fixed, according as our lines of glance are fixed relatively to the clouds or to the moon. So when, in a railway station, there is another train alongside of ours, and one of the trains begins to move, we can see either our own train or the other one moving, that train seeming fixed which is fixed relatively to our lines of glance.

## On the Torsion of the Molluscan Body. By J. D. F. Gilchrist, M.A., B.Sc., Ph.D. *Communicated by* Professor Ewart, F.R.S.

(Read January 21, 1895.)

There is strong evidence for supposing that the Mollusca are derived from a bilaterally symmetrical animal, having a median digestive tract and lateral excretory organs. This animal was further characterised by the physiological character of its dorsal integument, which had the property of secreting a peculiar substance—conchiolin. This, though in itself capable of affording some protection to the animal, becomes impregnated with carbonate of lime, thus providing the animal with a hard coating sufficient to resist the attacks of all but the most powerful of its enemies, while at the same time it forms a strong external skeleton to which the muscles of the body can be attached. This conchiolin, however, unlike chitin, does not seem to lend itself to the formation of jointed appendages or metameric segmentation, with which we are so familiar in another group, the Arthropoda.

These are the primary facts on which the general features of the Mollusca are mapped out, and to which they owe their well circumscribed character as a group. Other secondary factors come in which determine the classification of the sub-groups, and, as we shall see, tend to modify even the primary characters we have mentioned. These depend on the various modes in which the animal can utilise for the purpose of protection the calcareous plates or shells with which it is provided. One common mode is by a number of such plates along the dorsal surface, so arranged that even when the animal is detached from its support they are capable of being so folded together as to inclose the animal completely—a mode of protection similar to that adopted by the armadillo among Mammalia. Mollusca protected in this way belong to the group which has been named the *Polyplacophora.* Such an arrangement evidently does not interfere with the primitive bilateral symmetry of the animal, and this group approaches nearer to the type of the primitive Mollusc than any other.

It may happen, however, that the animal takes to other modes of life, or acquires other means of defence, and the old and somewhat inconvenient mode of protection is rendered superfluous, and is dispensed with. Thus we find a group—the *Aplacophora*—where in the adult all traces of a shell are lost.

Another well marked division of the Mollusca is constituted by those forms protected by two shells only. These are laterally placed, and by being closely approximated can afford a secure protection for the animal which lies in the space between them. Such an arrangement would be, of course, impracticable in a crawling animal, but is well adapted to the peculiar mode of feeding of this animal. Such a mode of protection has a modifying effect on the head and foot region, but the bilateral symmetry of the animal is still retained. This group might be called the *Diplacophora*. Some of these forms (*e.g.*, Galeomma) may be regarded as exhibiting a tendency towards an aplacophorous condition.

By far the greater majority of the Mollusca, however, are animals which possess, or have possessed, a single shell as a means of protection. This group might be called the *Monoplacophora*. The shell, being capable at all times of containing the animal, must necessarily be in the form of a gradually enlarging tube. In its growth it may increase equally all round its edge, in which case it assumes a conical shape; or it may grow more at one side than the other, in which case it has a coiled form of a definite geometrical shape. In animals which have a burrowing mode of life (Dentalium), or are free-swimming (Nautilus), the carrying of this heavy burden has little or no effect on the bilateral symmetry of the body; but in the case of the Gasteropoda or crawling forms, where the animal has to bear the weight of its shell, an entirely new factor is introduced, viz., a torsion of the body. At the side, usually the left, on which the shell bears most heavily, the gill, nephridium, &c., either become wholly atrophied, or change their position so as to lie on the opposite side. There is a corresponding change in the organs of the right side, which pass over towards the left; and in the anus, which now comes to occupy an anterior position. Spengel has pointed out a striking confirmation of this in the crossing of the pleuro-visceral nerves brought about by this torsion.

In this group—the Monoplacophora—we find, as we did in the

Polyplacophora, the same phenomenon of the disappearance of the shell in forms which, from embryological and anatomical evidence, we know to have been once possessed of a shell—as in the groups of the Cephalopods, the Pteropods, the Pulmonates, and the Opisthobranchs. The indirect reasons for dispensing with a shell have probably been the acquiring of other means of safety—such as new organs of defence and offence, protective resemblance, &c.

The question arises as to what becomes of the torsion of the body in those forms in which its mechanical cause (the shell) is removed. The chief object of the present paper is to note the results which have followed this process, which is none the less a true physiological experiment that it has been performed by nature. Is this twisting of the body undone? Does the organism regain its primitive symmetry? Do the atrophied organs reappear?

The group of the Opisthobranchs is peculiarly suited for this investigation, as we find in it all stages from the shelled to the completely shell-less forms. In describing the various changes in the topography which follow the removal of the shell, chief attention will be paid to the position of the pallial cavity (shaded part in figures), the position of the gill (indicated by its line of attachment to the body), the position of the heart, and of the nephridial, genital, and anal apertures. A description of the nephridium itself would have been of great interest, but enough material was not at hand for this investigation. Due allowance must be made for possible contraction and contortion caused by fixing reagents. The specimens examined were chiefly from Naples: some of the rarer forms were kindly sent me by Mr J. P. Hill, University of Sydney, and were useful for comparison.

We may note, in the first place, certain general features of the organism in the shelled forms. There are two parts of the body which must bear a definite relation to the shell, and therefore to each other, viz., the mantle which secretes the shell and the foot, which, along with the whole animal, can be withdrawn into the shell for protection. When, however, the shell disappears, these two organs are, as it were, set free to develop in any direction, and there is practically no limit to their possible transformation. As a matter of fact, we do find very different lines of development taken in different forms. This has an important bearing on the topography of the body.

Fig. 1 represents the relative positions of the organs in the pallial

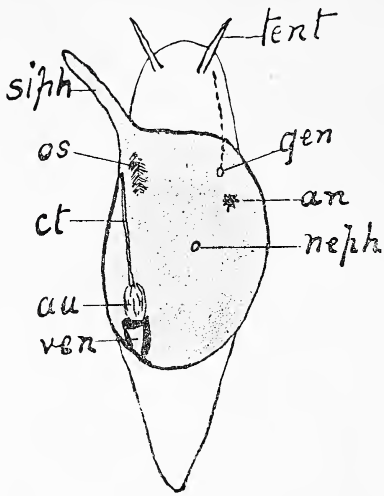



Fig. 1.

cavity of a Prosobranch. The figure is diagrammatic. It must be remembered that the gill and osphradium are attached to the roof of the mantle cavity, and are thus drawn up into a position in which the osphradium is to the actual left of the gill. It will be noted that the gill and osphradium are situated to the left in the mantle cavity, and that the latter is in a direct line, with a funnel-like prolongation of the mantle (the siphon). This left side may be regarded as the inhalent side, and the right side as the excretory; and it would appear that the necessity (endeavour?) of getting a pure supply of water, and the formation of an external organ (siphon) to appreciate the direction of stimuli, have something to do with the position of this organ. As a result we have the pallial cavity brought forward and to the left.

These considerations will explain the condition which we find in the first Opisthobranch we take up, viz., Bulla. There is no turreting of the shell, a condition associated with the position of the mantle. Fig. 2 will show that the position of the pallial organs is quite different from that in the case just mentioned. The gill is now turned towards the right side, and the heart lies obliquely to the body, almost in the reverse direction to that in fig. 1. The most noteworthy change is, however, in the osphradium, both as to its structure and position. It has become quite a rudimentary organ, its function being probably taken up by a new set of organs altogether, viz., two patches of olfactory epithelium on either side of the head region (fig. 2, *rhin.*). We shall see that these new organs become more highly developed, and finally appear as well differentiated "rhinophora," with which the simpler form is probably homologous. Meanwhile the osphradium becomes completely rudimentary, and finally disappears altogether. It will be seen from

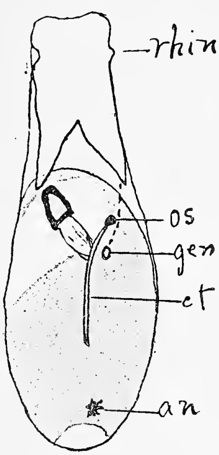



Fig. 2.

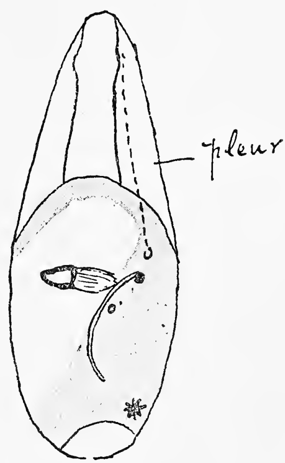



Fig. 3.

the figure that the genital opening lies well within the pallial cavity, and that the anus occupies a posterior position; the mantle itself is also of less extent than in fig. 1.

The next form we take up is Acera (fig. 3). The foot here is much larger, and extends laterally in the form of distinct "pleuropodia." The gill faces slightly more towards the posterior, but the heart does not point in the posterior direction so much as in Bulla. This is perhaps due to the fact that the pallial cavity may be firmly closed by a sphincter-like contraction, in which the anterior limb of the gill attachment is drawn away from the heart and sidewards. This mechanism may be observed in the living animal, the slightest touch at the entrance of the pallial cavity being sufficient to cause it to be abruptly closed. Perhaps also to be explained by this is the fact that the genital opening has now considerably changed its position, so that, when this contraction occurs, it is completely shut out from the pallial cavity. This is the first indication of the wide separation of the genital opening from the others, which is so marked a feature of many other forms among the Opisthobranchs. In Acera, also, we find the first trace of the disappearance of the shell. It is thin and membraneous. This condition is evidently to be attributed to the change in the position of its secreting organ, the mantle,—for it, like the foot and the animal generally, shows a tendency to come out of the shell. It lies partially reflected over the outside of the shell. We do not therefore necessarily require to call in the aid of natural selection to account for the disappearance of the shell.

Such forms as Doridium (fig. 4) or Philine (practically fig. 4) are illustrations of further progress in the same direction. Here the

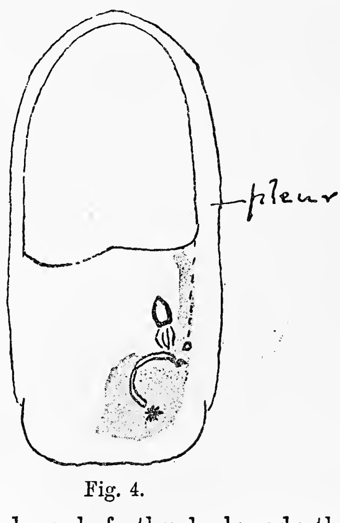



Fig. 4.

pallial organs are turned much further backward; the shell is now completely enveloped by the mantle; the visceral region is about equal to the cephalic region.

The forms we have been considering are all animals in which the first stages only of the disappearance of the shell are seen, viz., it becomes enveloped by the mantle; the coiling becomes less marked and wider, a fact no doubt due to the diminished rate of growth of the shell in proportion to the growth of the animal; and this, again, is connected with the enveloping by the mantle.

The fact that there is present a shell still capable of containing part of the viscera has an important bearing on the position of the mantle cavity. Viewing the animal from above, as in the diagrams, it would appear as if the mantle and organs associated with it were shifted to the posterior of the body. This, however, is not to be considered as actually the case if we are to compare these forms with others in which the shell is quite flattened out, and where the viscera formerly contained in the coiled shell now descend into the foot. Here the shell and visceral mass have merely fallen or been dragged backwards, and there is not a true morphological translation of parts. This position, which it is difficult to regard as other than the result of a purely mechanical strain, is also reflected in the dragging backward and twisting of the heart (see figs.). The habits and environment of the animals are quite in keeping with this, as they are found (especially Doridium and Philine) crawling amongst debris and mud or sand.

Before leaving the Cephalaspidea we may mention a form—Gasteropteron—belonging to this group, but which does not fit in very well either to this division or those following, owing to the fact that it is adapted to a pelagic or semipelagic mode of life. It presents nothing of much interest in the torsion of the body. The pallial organs lie on the right side; the shell and mantle are much reduced, especially the former.

With the exception of this form, we have found that the shell in the Cephalaspidea is still an important structure to which the rest of the organism must be conformed. When, however, we pass over into the group where the shell is quite flattened out or disappears, these two organs—the mantle and the foot—begin to show peculiar specialisations, and become, as the shell formerly was, important factors in determining the topography of the body. There are two extremes:—1st, the foot may be much enlarged, and specialised into locomotory and respiratory organs, while the mantle remains small or disappears; 2nd, or *vice versa*, the mantle may enlarge and the foot remain stationary in its development.

Taking first the forms in which the foot preponderates, we come to a number of molluscs of which Aplysia is a type. The forms examined were various species of Aplysia, Dolabrifera, Dolabella, Notarchus, and Lobiger. These do not differ in any important detail as to the position of the pallial cavity, and fig. 5 (of Aplysia)

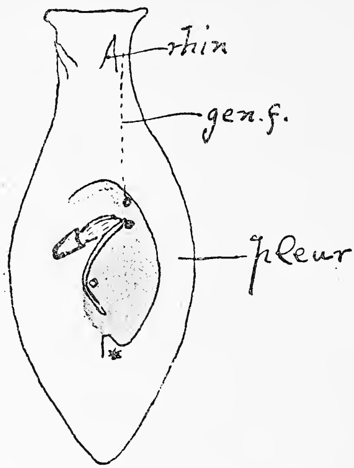



Fig. 5.

will serve to give a general idea of the position of the pallial organs of the whole. These are all situated on the dorsal part of the animal, in contrast to the previous group, where they had a tendency to be turned towards the right side of, and even partly under, the visceral mass. There is here no dragging backwards of any part, as indicated specially by the position of the heart. In Aplysia the pallial organs are crowded into the middle dorsal position by the surrounding pleuropodia, thus approaching a well-known Nudibranch arrangement (fig. 6). This is even more marked in Dolabrifera, where the pleuropodia (more particularly the left) tend to disappear, and the "pallial complex" is much reduced, as in the Nudibranchs, the resemblance to which is still further increased by the posterior position of the mantle cavity. Several species of Aplysia examined showed, on the other hand, transition stages between the position met with in the Cephalaspidea and that described here for Aplysia limacina, A. depilans, &c. This was found to be the case in A. piperata, A. brasiliana, &c.

Dolabella and Notarchus may be regarded as specialisations in the opposite direction where the pleuropodia, instead of climbing up the side of the animal as it were, increase so much both above and below the viscera as to envelop them like a sac.

Lobiger is interesting, as showing a tendency of the pleuropodia to divide into lobes, perhaps foreshadowing the numerous dorsal processes of the Nudibranchs.

There is nothing calling for special notice in the arrangement in Umbrella, which is adapted for a special mode of life—lying buried in mud and debris. The most striking features are the large foot and the development of the mantle edge into finger-like sensory and glandular processes, instead of being reflected over the shell. The great extension of the gill is also noteworthy, and the transformation of the anterior part of it prepare us for the greater transformation we meet with in the Nudibranchs.

We have been following the line of development in which the anal, nephridial, and genital opening, along with the gill, tend to take up a median and posterior position, this being obviously in coordination with the growth of the pleuropodia (foot). A gap in the series occurs here; and it is not till we pass over into the Nudi-

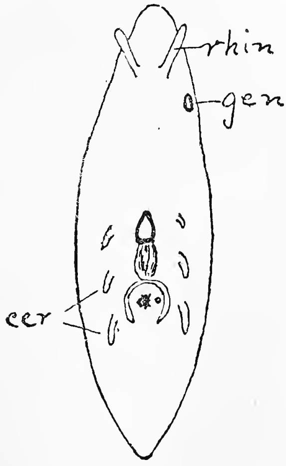



Fig. 6.

branchs that we find further change in this direction, and these changes are then so radical as to throw doubts on the homology of some of the parts. For instance, in Polycera (fig. 6) the anal and nephridial openings are practically in the middle line, and are surrounded by the gill; but so different is the structure of this gill that it might well be a new formation. Again, we may ask are the dorsal processes or cerata (*cer.*) the homologues of the pleuropodia? The genital opening is now near the head region, in the position of the anterior end of the genital furrow (fig. 5, *gen. f.*) in previous forms.

In the case of Fiona (fig. 7) the gill has disappeared, and the function of respiration seems to be entirely performed by the dorsal processes, which have greatly increased in number.

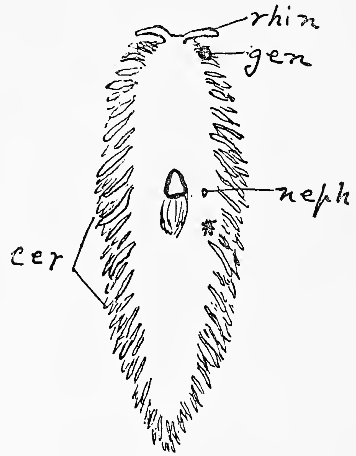



Fig. 7.

In both these forms we meet, for the first time, with a total disappearance of the osphradium along with the pallial cavity. There are, however, well developed rhinophora in the head region.

As to the homology of parts here suggested, it cannot be maintained with absolute certainty; but the line of development we have been following in the Tectibranchs has prepared us for every one of the transformations we now meet with in these Nudibranchs.

Another line of development is, however, to be traced from the Acera type. The *mantle* may preponderate in development, causing a considerable displacement of the pallial organs, not as before, towards the dorsal surface, but rather laterally and ventrally. The mantle cavity proper disappears, and the mantle projects on all sides, leaving a groove between it and the foot. The pallial organs lie in this groove on the right side, and show some important variations in position.

Pleurobranchus (fig. 8) may be taken as a type of this arrangement. The various pallial organs are arranged practically in a line. There are well developed rhinophora; no osphradium, but in its

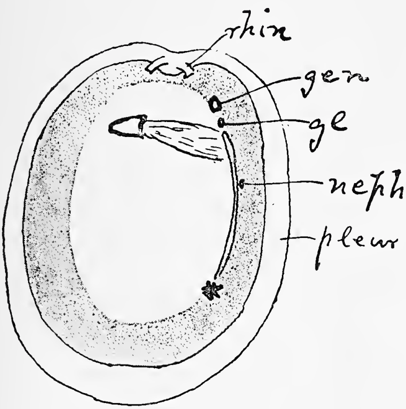



Fig. 8.

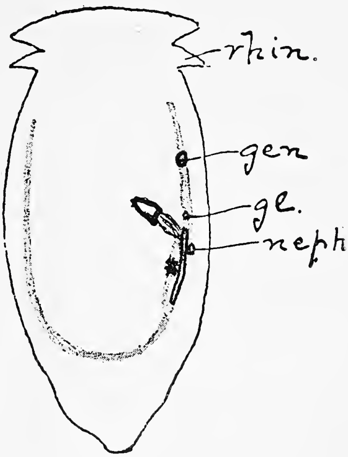



Fig. 9.

place an opening (*gl.*) which leads into a glandular sac of unknown function. Nephridial opening and gill are situated alongside of each other, while the anus is just behind the gill.

A closely allied form, Pleurobranchea (fig. 9), differs curiously in some details. The foot shows no broadening out as in Pleurobranchus: the mantle is reduced to a mere ledge round the body, and the shell has been found to exist only in a very reduced condition. The genital opening is further removed from the head region and the gland above mentioned; gill and nephridial opening are situated posteriorly, and occupy a striking position relatively to each other. As will be seen from the figure, the gill is now the leading organ in the backward retreat. It is difficult to suppose this to be the result of a "favourable variation," as it is just between two excretory organs. Perhaps this has something to do with the fact that in passing over to the Nudibranchs we find a form in which the gill has disappeared, while the respiratory function is taken up by a number of lamellæ, into which the edge of the mantle has split up (compare a similar phenomenon in Patella). This is the case in Pleurophyllidia, the relative position of whose pallial organs agrees with that in Pleurobranchea. From this the further step towards the condition in Tritonia is easy—where the mantle processes become divided up into typical Nudibranch appendages, the mantle organs still retaining the lateral linear position.

## CONCLUSIONS.

We may sum up what we have now arrived at concerning the torsion of the Molluscan body looked at from this negative point of view of the untwisting after torsion. With the disappearance of the osphradium there is a turning backwards of the pallial complex (Bulla), and with the falling backwards of the visceral mass there is a dragging backwards of the heart, and further retreat of the pallial organs, with diminution of the pallial cavity (Philine). When the shell becomes flattened out or rudimentary it ceases to exercise any distorting influence on the body, and the foot and mantle become the modifying factors, leading to characteristic variations. In the Nudibranchs the original bilateral symmetry of the Mollusca (*cf.* fig. 6) is apparently resumed, but the lost organs of the one side do not again reappear.

It will have been noted that in all these variations there is usually present an obvious mechanical cause (coming into play only during post-embryonic life), such as the inclination of the heavy shell to one side, causing a one-sided pallial cavity; the dragging forward of the mantle by a sensory organ, and backward by excretory organs, both under the control of and exercised by the animal; the falling backwards of the shell and visceral mass, due to mode of life; the consequent dragging backwards of the heart; the becoming rudimentary of the shell, due to change in the position of its generating organ, the mantle. Natural selection with indefinite variation will, of course, account for these changes, as it will for any change that cannot be proved to be positively injurious to the animal or the species. Such a proof is perhaps impossible, yet every case of possible definite variation with a possible mechanical cause increases the probability against the latter solution being applicable to all cases.

Finally, we may note the significance of the indications we have met with in the Tectibranchiata, that there are two lines of development, according to the relative preponderance of the mantle or foot. We have traced these through intermediate forms to what appear to be two corresponding groups in the Nudibranchiata. Judging from this evidence alone, therefore, we would presume that the cerata are neither the homologues of the mantle (Pelseneer) nor of the foot (Herdman), but that they may be the one or the other in different cases, and there is no reason why they should not sometimes be both. Professor Herdman has shown that the cerata are not innervated exclusively by the pleuro-visceral nerves (mantle nerves), as was previously supposed, nor by the pedal nerves (pleuropodial nerves), but that they are innervated from the one or the other, or both sources, in different forms. He infers from this that here innervation is not to be trusted as a guide to homology of parts. If, however, these apparently conflicting facts be taken in conjunction with the lines of development in the immediate ancestors of the group, it will be apparent that they afford valuable confirmatory evidence for the conclusions here arrived at, viz., that the various forms in the Nudibranchiata are to be referred back for interpretation to the Tectibranchiata, where there may be preponderance of the mantle on the one hand or the foot on the other, or maybe equal development of both, for it is not to be supposed that the two lines of development are the only alternatives, or even that they are exclusive of each other (Doris?). Other variations certainly exist in the Nudibranchiata, only a few forms of which we have considered.

[EXPLANATION

## EXPLANATION OF FIGURES.

Positions are given as viewed from above, except (as in the osphradium, fig. 1, and the nephridial opening, fig. 5) where this does not indicate true morphological position, for reasons explained in the text. The shaded part indicates the extent of the pallial cavity. The lettering in all the figures is the same, viz. :—

*an.*, anal opening.
*au.*, auricle.
*cer.*, cerata.
*ct.*, ctenidium.
*gen.*, genital opening.
*gen. f.*, genital furrow.
*gl.*, gland.
*neph.*, nephridial opening.
*os.*, osphradium.
*pleur.*, pleuropodia.
*rhin.*, rhinophora.
*siph.*, siphon.
*tent.*, tentacles.
*ven.*, ventricle.

Fig. 1. A siphonate Prosobranch.
Fig. 2. Bulla.
Fig. 3. Acera.
Fig. 4. Doridium.
Fig. 5. Aplysia.
Fig. 6. Polycera.
Fig. 7. Fiona.
Fig. 8. Pleurobranchus.
Fig. 9. Pleurobranchea.

## Further Note on a Problem of Sylvester's in Elimination. By Thomas Muir, LL.D.

(Read May 6, 1895.)

1. A first note on this subject, viz., the elimination of $x$, $y$, $z$ from the equations

$$\left.\begin{array}{l} Ay^2 - 2C'xy + Bx^2 = 0 , \\ Bz^2 - 2A'yz + Cy^2 = 0 , \\ Cx^2 - 2B'zx + Az^2 = 0 , \end{array}\right\}$$

was communicated to the Society in April 1892. By reason of my immediate departure for South Africa, the manuscript was not prepared for the press, and the paper was consequently not printed until this year. In the meanwhile, both Professor Tait and Lord M'Laren had written notes on the subject, under the impression, I fear, that the object of my original paper was to obtain in any way an easy solution of the problem.

2. The method of elimination which Professor Tait employs (*Proc.*, xix. pp. 131, 132) is the so-called Method of Symmetric Functions. He puts the equations in the form

$$\left.\begin{array}{l} \xi^2 - 2e_3\xi\eta + \eta^2 = 0 \\ \eta^2 - 2e_1\eta\zeta + \zeta^2 = 0 \\ \zeta^2 - 2e_2\zeta\xi + \xi^2 = 0 \end{array}\right\} ,$$

solves as it were for $\xi/\eta$, $\eta/\zeta$, $\zeta/\xi$, and substitutes in the identity

$$\frac{\xi}{\eta} \cdot \frac{\eta}{\zeta} \cdot \frac{\zeta}{\xi} = 1 .$$

Lord M'Laren's method (*Proc.*, xix. pp. 264, 265) is the same: but he solves for $\xi/\eta$, $\zeta/\eta$ in the first and second equations, and substitutes in the third. The eliminant is obtained as a square in both cases, although in the latter this is overlooked.

3. If it had formed part of my plan to go outside the dialytic

method, I might have given a solution founded on the same principle as these, but much simpler by reason of the fact that the particular form of the equations renders the actual solution of them unnecessary. For, as they stand, each equation may be viewed as an assertion regarding the sum of its roots; and, writing them *explicitly* in this form, we have

$$\left.\begin{aligned} \frac{\xi}{\eta}+\frac{\eta}{\xi}&=2e_3 \\ \frac{\eta}{\zeta}+\frac{\zeta}{\eta}&=2e_1 \\ \frac{\zeta}{\xi}+\frac{\xi}{\zeta}&=2e_2 \end{aligned}\right\}$$

and therefore by multiplication

$$\begin{aligned} 8e_1e_2e_3 &= \frac{\eta^2}{\xi^2}+\frac{\xi^2}{\eta^2}+\frac{\zeta^2}{\xi^2}+\frac{\xi^2}{\zeta^2}+\frac{\eta^2}{\zeta^2}+\frac{\zeta^2}{\eta^2}+2, \\ &= \left(\frac{\eta}{\xi}+\frac{\xi}{\eta}\right)^2-2+\left(\frac{\zeta}{\xi}+\frac{\xi}{\zeta}\right)^2-2+\left(\frac{\eta}{\zeta}+\frac{\zeta}{\eta}\right)^2, \\ &= 4e_3{}^2+4e_1{}^2+4e_2{}^2-4, \end{aligned}$$

which is the desired result.

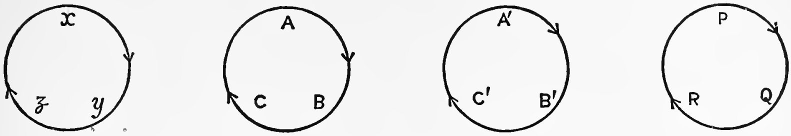
My printed paper shows, however, that I had a more important object than this in view.

4. The first fresh point to which I now wish to direct attention is the question of the occurrence of the eliminant in the form of a *square*. In elucidation of it we cannot do better than take a more general set of equations than Sylvester's, and observe the way in which the eliminant of the former degenerates into that of the latter.

Such a set of equations is

$$\left.\begin{aligned} Ay^2-2C'xy+Qx^2&=0 \\ Bz^2-2A'yz+Ry^2&=0 \\ Cx^2-2B'zx+Pz^2&=0 \end{aligned}\right\}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (a)$$

where in the third terms P, Q, R take the places of the A, B, C of the original set, and where each equation is derived from another of the set by changing the letters in accordance with the diagrams




5. From ($\alpha$) we first obtain the set corresponding to Sylvester's derived set for the simpler problem, viz.,

$$\left.\begin{array}{l} 2\mathrm{CQA}'x^2 + (\mathrm{ABC}+\mathrm{PQR})yz - 2\mathrm{BCC}'zx - 2\mathrm{QRB}'xy = 0 \\ 2\mathrm{ARB}'y^2 - 2\mathrm{RPC}'yz + (\mathrm{ABC}+\mathrm{PQR})zx - 2\mathrm{CAA}'xy = 0 \\ 2\mathrm{BPC}'z^2 - 2\mathrm{ABB}'yz - 2\mathrm{PQA}'zx + (\mathrm{ABC}+\mathrm{PQR})xy = 0 \end{array}\right\} (\beta)$$

the mode of derivation being indicated by the operational formula

$$\mathrm{PB}z^2(\alpha_1) - \mathrm{PQ}x^2(\alpha_2) - \mathrm{BA}y^2(\alpha_3) = -xy(\beta_3)\,.$$

6. Another similar set is got by solving ($\alpha$) for $x^2$, $y^2$, $z^2$ in terms of $yz$, $zx$, $xy$, viz.,

$$\left.\begin{array}{l} (\mathrm{ABC}+\mathrm{PQR})x^2 = 2\mathrm{RPC}'xy + 2\mathrm{ABB}'zx - 2\mathrm{APA}'yz \\ (\mathrm{ABC}+\mathrm{PQR})y^2 = 2\mathrm{BCC}'xy - 2\mathrm{BQB}'zx + 2\mathrm{PQA}'yz \\ (\mathrm{ABC}+\mathrm{PQR})z^2 = -2\mathrm{CRC}'xy + 2\mathrm{QRB}'zx + 2\mathrm{CAA}'yz \end{array}\right\} \quad . \quad . \quad (\gamma)$$

7. Substituting in ($\beta$) for $xy$, $yz$, $zx$ by means of ($\alpha$) we obtain three equations in $x^2$, $y^2$, $z^2$, viz.,

$$\begin{array}{l} 2\mathrm{A}'(2\mathrm{CQA}'\mathrm{B}'\mathrm{C}' - \mathrm{BC}^2\mathrm{C}'^2 - \mathrm{B}'^2\mathrm{Q}^2\mathrm{R})x^2 \\ + \mathrm{B}'\{(\mathrm{ABC}+\mathrm{PQR})\mathrm{RC}' - 2\mathrm{AQRA}'\mathrm{B}'\}y^2 \\ + \mathrm{C}'\{(\mathrm{ABC}+\mathrm{PQR})\mathrm{BB}' - 2\mathrm{BCPA}'\mathrm{C}'\}z^2 = 0 \end{array} \quad . \quad . \quad . \quad (\delta)$$

and two others.

8. From ($\alpha$), as we have seen, $x^2$ can be expressed in terms of $yz$, $zx$, $xy$, so that substitution in ($\beta_1$) gives us a set in $yz$, $zx$, $xy$, viz.,

$$\begin{array}{l} 2\mathrm{QR}\{(\mathrm{ABC}+\mathrm{PQR})\mathrm{B}' - 2\mathrm{CPA}'\mathrm{C}'\}xy \\ + \quad \{4\mathrm{ACPQA}'^2 - (\mathrm{ABC}+\mathrm{PQR})^2\}yz \\ + 2\mathrm{BC}\{(\mathrm{ABC}+\mathrm{PQR})\mathrm{C}' - 2\mathrm{AQA}'\mathrm{B}'\}zx = 0 \end{array} \quad . \quad . \quad (\epsilon)$$

9. Proceeding in a converse way, viz., using ($\beta_1$) and ($\beta_3$) to give us $x^2$ and $z^2$, each expressed in terms of $yz$, $zx$, $xy$, and then substi-

tuting in ($\alpha$), we obtain another set in the same variables. The result is

$$\begin{aligned} &\mathrm{B}\{2\mathrm{AQA'B'} - \mathrm{C}(\mathrm{ABC}+\mathrm{PQR})\}\,yz \\ &+2(\mathrm{B^2CC'^2} - 2\mathrm{BQA'B'C'} + \mathrm{PQ^2A'^2})zx \\ &+\mathrm{Q}\{2\mathrm{BRB'C'} - \mathrm{A'}(\mathrm{ABC}+\mathrm{PQR})\}\,xy = 0 \quad . \quad . \quad . \quad (\zeta) \end{aligned}$$

and two others.

10. Lastly, by eliminating $xy$ from ($\beta_3$) and ($\gamma_3$), we have the equations in $x$, $y$, $z$, viz.,

$$\begin{aligned} &2\mathrm{QR}\{(\mathrm{ABC}+\mathrm{PQR})\mathrm{B'} - 2\mathrm{CPA'C'}\}x \\ &+2\mathrm{AC}\{(\mathrm{ABC}+\mathrm{PQR})\mathrm{A'} - 2\mathrm{BRB'C'}\}y \\ &+ \quad \{4\mathrm{BCPRC'^2} - (\mathrm{ABC}+\mathrm{PQR})^2\}z = 0 \quad . \quad . \quad . \quad (\eta). \end{aligned}$$

and two others.

11. From these sets of equations several forms of the eliminant are obtainable. All of them are a little forbidding in appearance, and the calculation of the expansion of them may seem troublesome; but if attention be paid to the fact that any term leads to two others by means of cyclical substitution, the work will be found comparatively easy. The same fact makes it also desirable to use a symbol, such as $\mathring{\Sigma}$, for shortly expressing cyclical sums of three terms. Thus

$$\mathring{\Sigma}\mathrm{BC}\cdot\mathrm{RP}\cdot\mathrm{C'^2}$$

would stand for

$$\mathrm{BC}\cdot\mathrm{RP}\cdot\mathrm{C'^2} + \mathrm{CA}\cdot\mathrm{PQ}\cdot\mathrm{A'^2} + \mathrm{AB}\cdot\mathrm{QR}\cdot\mathrm{B'^2},$$

where, it may be observed, the multiplication points are used to separate the letters of one cycle from those of another.

12. Perhaps, for our present purpose, the best form of writing the eliminant, as thus calculated, is

$$\begin{aligned} &(\mathrm{ABC}+\mathrm{PQR})^4 + 64\mathrm{ABC}\cdot\mathrm{PQR}\cdot\mathrm{A'^2B'^2C'^2} + 32\mathrm{ABC}\cdot\mathrm{PQR}\cdot\mathrm{A'B'C'}(\mathrm{ABC}+\mathrm{PQR}) \\ &\quad - 8(\mathrm{ABC}+\mathrm{PQR})(4\mathrm{A'B'C'}+\mathrm{ABC}+\mathrm{PQR})\mathring{\Sigma}(\mathrm{AB}\cdot\mathrm{QR}\cdot\mathrm{B'^2}) \\ &\quad + 16(\mathrm{A^2B^2C^2}+\mathrm{P^2Q^2R^2})\mathring{\Sigma}(\mathrm{A}\cdot\mathrm{Q}\cdot\mathrm{A'B'}) \\ &\quad + 16\mathring{\Sigma}(\mathrm{A^2B^2}\cdot\mathrm{Q^2R^2}\cdot\mathrm{B'^4}) \\ &\quad - 8(\mathrm{ABC}+\mathrm{PQR})(\mathrm{ABC}-\mathrm{PQR})^2\mathrm{A'B'C'}. \end{aligned}$$

It is seen to be of the 12th degree, so that the seemingly small increase in generality made in Sylvester's equations by putting in one place P, Q, R for A, B, C raises the degree of the eliminant from the 3rd to the 12th. This is the more noteworthy, because had the full increase in generality been made—that is to say, had the given equations been any three ternary quadrics whatever—the degree of the eliminant would have been no higher.

13. Putting now P, Q, R = A, B, C, we first notice that the last term $-8(ABC+PQR)(ABC-PQR)^2A'B'C'$ disappears, and then that the remaining portion becomes

$$\begin{aligned}16A^4B^4C^4 &+ 64A^2B^2C^2\cdot A'^2B'^2C'^2 + 64A^3B^3C^3\cdot A'B'C'\\ &- 32ABC(ABC+2A'B'C')(AB\cdot BC\cdot B'^2 + BC\cdot CA\cdot C'^2 + CA\cdot AB\cdot A'^2)\\ &+ 32A^2B^2C^2(A\cdot B\cdot A'^2B'^2 + B\cdot C\cdot B'^2C'^2 + C\cdot A\cdot C'^2A'^2)\\ &+ 16(A^2B^2\cdot B^2C^2\cdot B'^4 + B^2C^2\cdot C^2A^2\cdot C'^2 + C^2A^2\cdot A^2B^2\cdot A'^4)\,.\end{aligned}$$

Of this $16A^2B^2C^2$ is a factor, and the cofactor is

$$\begin{aligned}A^2B^2C^2 &+ 4A'^2B'^2C'^2 + 4ABC\cdot A'B'C'\\ &- 2(ABC+2A'B'C')(BB'^2+CC'^2+AA'^2)\\ &+ 2(AB\cdot A'^2B'^2 + BC\cdot B'^2C'^2 + CA\cdot C'^2A'^2)\\ &+ (B^2B'^4 + C^2C'^4 + A^2A'^4)\,,\end{aligned}$$

which is easily seen to be the square of

$$ABC + 2A'B'C' - AA'^2 - BB'^2 - CC'^2\,.$$

14. But now that the more general eliminant has been calculated, a most important property of it makes its appearance, viz., that it is symmetrical with respect to the interchange

$$\begin{pmatrix}A, & B, & C\\ Q, & R, & P\end{pmatrix}.$$

An examination of the original equations shows (see § 17) that this is as it ought to be, and the hope is raised that a determinant form of the eliminant may be discovered which will bear the said symmetry clearly on the face of it. After many trials I have succeeded in obtaining this very interesting determinant form, the equations from which it is derived being those which involve the compound variables

$$yz^2\,,\quad zx^2\,,\quad xy^2\,,\quad xyz\,.$$

15. Taking the four equations

$$\left.\begin{array}{l} Ay^2z - 2C'xyz + Qx^2z = 0 \\ Bz^3 - 2A'yz^2 + Ry^2z = 0 \\ Cx^2z - 2B'z^2x + Pz^3 = 0 \\ Bxz^2 - 2A'xyz + Rxy^2 = 0 \end{array}\right\}$$

which are got from the original equations simply by multiplying by $z$ and $x$, we see that $y^2z$, $z^3$, $z^2x$ may be dialytically eliminated. Doing this we have

$$\left|\begin{array}{cccc} . & A & . & Qx^2z - 2C'xyz \\ B & R & . & -2A'yz^2 \\ P & . & -2B' & Cx^2z \\ . & . & B & Rxy^2 - 2A'xyz \end{array}\right| = 0,$$

or

$$2APA'yz^2 + (ABC + PQR)zx^2 + 2ARB'xy^2 - (2RPC' + 4AA'B')xyz = 0 .$$

From this, by the cyclical substitution, two other equations in the same variables are obtained, and therefore only one more is wanted for elimination. Now the original equations may be written

$$\left.\begin{array}{l} Ay^2 - C'xy = C'xy - Qx^2 \\ Bz^2 - A'yz = A'yz - Ry^2 \\ Cx^2 - B'zx = B'zx - Pz^2 \end{array}\right\}$$

and thus by multiplication we have

$$\text{or } (Ay - C'x)(Bz - A'y)(Cx - B'z) = (C'y - Qx)(A'z - Ry)(B'x - Pz)$$

$$(ABC + PQR - 2A'B'C')xyz + \mathring{\Sigma}(CC'A' - QRB')x^2y + \mathring{\Sigma}(RB'C' - CAA')xy^2 = 0 \quad \text{(i)}.$$

Again, from the original equations it is clear that

$$\mathring{\Sigma}(B'Ry - 3A'B'z - A'Cx)(Ay^2 - 2C'xy + Qx^2) = 0 ,$$

and this, after performing the operations indicated, we find to be

$$18A'B'C'xyz + \mathring{\Sigma}(QRB' - CC'A')x^2y - \mathring{\Sigma}(5RB'C' + CAA')xy^2 = 0 \; . \; . \; . \; . \quad \text{(ii)}.$$

But the coefficients of $x^2y$ in (i) and (ii) differ only in sign, consequently by addition we have the equation of the desired form, viz.,

$$-\mathring{\Sigma}(4RB'C' + 2CAA')x^2y + (ABC + PQR + 16A'B'C')xyz = 0 .$$

The four equations thus obtained therefore are

$$\left.\begin{array}{llll} (ABC+PQR)yz^2+ & 2CQA'zx^2+ & 2CRC'xy^2- & (2QRB'+4CC'A')xyz=0 \\ 2APA'yz^2+ & (ABC+PQR)zx^2+ & 2ARB'xy^2- & (2RPC'+4AA'B')xyz=0 \\ 2BPC'yz^2+ & 2BQB'zx^2+ & (ABC+PQR)xy^2- & (2PQA'+4BB'C')xyz=0 \\ -(2ABB'+4PC'A')yz^2 & -(2BCC'+(4QA'B')zx^2 & -(2CAA'+4RB'C')xy^2 & +(ABC+PQR+16A'B'C')xyz=0 \end{array}\right\},$$

and the eliminant derived from them is

$$\begin{vmatrix} ABC+PQR & 2CQA' & 2CRC' & 2QRB'+4CC'A' \\ 2APA' & ABC+PQR & 2ARB' & 2RPC'+4AA'B' \\ 2BPC' & 2BQB' & ABC+PQR & 2PQA'+4BB'C' \\ 2ABB'+4PC'A' & 2BCC'+4QA'B' & 2CAA'+4RB'C' & ABC+PQR+16A'B'C' \end{vmatrix}.$$

16. The great interest attaching to this very neat form of the eliminant lies in the fact that the effect of making the interchange

$$\begin{pmatrix} A, & B, & C \\ Q, & R, & P \end{pmatrix}$$

is simply to change rows into columns and *vice versa*, so that its symmetry with respect to the said interchange is evident at a glance.

Still more interesting is the degenerate form obtained from it for Sylvester's case. After making the requisite substitution P, Q, R = A, B, C, the factors 2A, 2B, 2C, 2, C, A, B can be struck out, and the result is

$$\begin{vmatrix} B & A' & C' & BB'+2C'A' \\ A' & C & B' & CC'+2A'B' \\ C' & B' & A & AA'+2B'C' \\ BB'+2C'A' & CC'+2A'B' & AA'+2B'C' & ABC+8A'B'C' \end{vmatrix}.$$

The result to be expected, however, is

$$\begin{vmatrix} B & A' & C' \\ A' & C & B' \\ C' & B' & A \end{vmatrix}^2,$$

and thus we have the curious identity

$$\begin{vmatrix} B & A' & C' & BB'+2C'A' \\ A' & C & B' & CC'+2A'B' \\ C' & B' & A & AA'+2B'C' \\ BB'+2C'A' & CC'+2A'B' & AA'+2B'C' & ABC+8A'B'C' \end{vmatrix} = \begin{vmatrix} B & A' & C' \\ A' & C & B' \\ C' & B' & A \end{vmatrix}^2$$

—that is to say, *we have found an axisymmetric determinant which is the square of one of its own primary coaxial minors.*

To establish this identity *directly* we have only to subtract from the 4th row B′ times the 1st row, C′ times the 2nd row, and A′ times the 3rd row; for by this operation the first three elements of the 4th row vanish, and the fourth element becomes

$$\begin{vmatrix} B & A' & C' \\ A' & C & B' \\ C' & B' & A \end{vmatrix}.$$

17. If, instead of choosing the variables $yz^2$, $zx^2$, $xy^2$, $xyz$, we had taken the similar set $x^2y$, $y^2z$, $z^2x$, $xyz$, the result would have been practically the same; for the set of equations in the latter four variables is

$$\left.\begin{array}{llll} (ABC+PQR)x^2y + & 2APA'y^2z + & 2BPC'z^2x - & (2ABB'+4PC'A')xyz = 0 \\ 2CQA'x^2y + & (ABC+PQR)y^2z + & 2BQB'z^2x - & (2BCC'+4QA'B')xyz = 0 \\ 2CRC'x^2y + & 2ARB'y^2z + & (ABC+PQR)z^2x - & (2CAA'+4RB'C')xyz = 0 \\ -(2QRB'+4CC'A')x^2y & -(2RPC'+4AA'B')y^2z & -(2PQA'+4BB'C')z^2x & +(ABC+PQR+16A'B'C')xyz = 0 \end{array}\right\},$$

and the eliminant differs from that of the preceding paragraph in form only, the rows of the one being the columns of the other. The explanation of this is to be found in the fact that the interchange

$$\begin{pmatrix} A, & B, & C, & x, & y, & z \\ Q, & R, & P, & \frac{1}{x}, & \frac{1}{y}, & \frac{1}{z} \end{pmatrix}$$

leaves the original equations unaltered.

18. It might be thought that the equations connecting $x^3$, $y^3$, $z^3$, $xyz$ would lead to a simple symmetrical form of the eliminant. This, however, does not seem to be the case, the equations being

$$\left.\begin{array}{llll} (Q^2R\beta + CB\gamma)x^3 + & 2ARC'\beta y^3 + & 2BPB'\gamma z^3 - & 2A'\beta\gamma \,.\, xyz = 0 \\ 2CQC'\alpha x^3 + & (R^2P\gamma + A^2C\alpha)y^3 + & 2BPA'\gamma z^3 - & 2B'\gamma\alpha \,.\, xyz = 0 \\ 2CQB'\alpha x^3 + & 2ARA'\beta y^3 + & (P^2Q\alpha + B^2A\beta)z^3 - & 2C'\alpha\beta \,.\, xyz = 0 \\ 2CQA'x^3 + & 2ARB'y^3 + & 2BPC'z^3 + & (ABC+PQR+4A'B'C')xyz = 0 \end{array}\right\}$$

where $\alpha = 4A'^2 - BC$, $\beta = 4B'^2 - CA$, $\gamma = 4C'^2 - AB$, and the eliminant is consequently of the 18th degree.

19. Although the various sets of derived equations in the foregoing may seem to be obtained from the original set in haphazard

ways, such is not necessarily the case; and it is not difficult to indicate a general mode of procedure for obtaining them.

From the original equations, by multiplying each in succession by $x$, $y$, $z$, we obtain nine equations in the ten unknowns

$$x^3,\ y^3,\ z^3\ ;\ x^2y,\ y^2z,\ z^2x\ ;\ xy^2,\ yz^2,\ zx^2\ ;\ xyx\,.$$

The tenth equation wanted for the purpose of elimination Sylvester obtained * by writing the original equations in the form

$$\left.\begin{array}{lll} & (\mathrm{A}y-\mathrm{C}'x)y+(\mathrm{Q}x-\mathrm{C}'y)x & =0 \\ (\mathrm{B}z-\mathrm{A}'y)z+(\mathrm{R}y-\mathrm{A}'z)y & & =0 \\ (\mathrm{P}z-\mathrm{B}'x)z & +(\mathrm{C}x-\mathrm{B}'z)x & =0 \end{array}\right\}$$

and eliminating $z$, $y$, $x$, the result being

$$\begin{array}{l}(\mathrm{P}z-\mathrm{B}'x)(\mathrm{R}y-\mathrm{A}'z)(\mathrm{Q}x-\mathrm{C}'y) \\ +(\mathrm{B}z-\mathrm{A}'y)(\mathrm{A}y-\mathrm{C}'x)(\mathrm{C}x-\mathrm{B}'z)=0\,,\end{array}$$

or

$$\mathring{\Sigma}(\mathrm{CC'A'}-\mathrm{QRB'})x^2y+\mathring{\Sigma}(\mathrm{RB'C'}-\mathrm{CAA'})xy^2+(\mathrm{ABC}+\mathrm{PQR}-2\mathrm{A'B'C'})xyz=0$$

It is easily shown, however, that this is not the simplest form of the tenth equation. For from the seventh equation, by multiplying by A′B′, we have

$$\left.\begin{array}{ll} & \mathrm{AA'B'}y^2z+\mathrm{QA'B'}zx^2-2\mathrm{A'B'C'}xyz=0 \\ \text{and similarly} & \mathrm{BB'C'}z^2x+\mathrm{RB'C'}xy^2-2\mathrm{A'B'C'}xyz=0 \\ \text{and} & \mathrm{CC'A'}x^2y+\mathrm{PC'A'}yz^2-2\mathrm{A'B'C'}xyz=0 \end{array}\right\}$$

and consequently

$$\mathring{\Sigma}\mathrm{CC'A'}x^2y+\mathring{\Sigma}\mathrm{RB'C'}xy^2-6\mathrm{A'B'C'}xyz=0.$$

From this and Sylvester's equation by subtraction we have

$$-\mathring{\Sigma}\mathrm{QRB'}x^2y-\mathring{\Sigma}\mathrm{CAA'}xy^2+(\mathrm{ABC}+\mathrm{PQR}+4\mathrm{A'B'C'})xyz=0\,,$$

which, besides being much simpler, still retains the property of symmetry with respect to the interchange

$$\begin{pmatrix}\mathrm{A}, & \mathrm{B}, & \mathrm{C}, & x, & y, & z \\ \mathrm{Q}, & \mathrm{R}, & \mathrm{P}, & \dfrac{1}{x}, & \dfrac{1}{y}, & \dfrac{1}{z}\end{pmatrix}$$

and is thus unique.

Now, if we denote $x^3$, $y^3$, $z^3$, $x^2y$, . . . ., $xyz$ by 1, 2, 3, . . . , 10, it is seen that the ten equations respectively involve the variables

* For another mode, see § 15.

1, 4, 7
2, 5, 8
3, 6, 9
2, 4, 7
3, 5, 8
1, 6, 9
5, 9, 10
6, 7, 10
4, 8, 10
4, 5, 6, 7, 8, 9, 10.

Consequently, if we wish to obtain a relation connecting certain of the variables, we select $m+1$ of these equations involving the said variables and $m$ others, so that we may be able to eliminate the $m$ variables which are not desired and retain those which are. Thus, supposing we wish to obtain four equations involving 1, 2, 3, 10, as was the case in § 18, we take the 2nd, 3rd, 5th, 6th, 7th equations, which involve these variables, and four others, viz., 5, 6, 8, 9, and from these five equations eliminate the latter set of unknowns. One equation of the desired kind having thus been got, two others follow from it by cyclical substitution. The requisite fourth equation, which transforms into itself by the cyclical substitution, is got by taking the tenth equation along with the first six, and eliminating the six variables 4, 5, 6, 7, 8, 9.

20. The method applies not merely to the variables $x^3$, $y^3$, $z^3$, $x^2y$, . . . ., which are of the 3rd degree. For supposing the equations in $x^2$, $xy$, $yz$, $zx$ (see § 6) are wanted, we have only to seek for an equation in $x^3$, $x^2y$, $xyz$, $zx^2$, that is, in 1, 4, 10, 9, and, when it has been got, strike out the common factor $x$, and derive the two other equations by cyclical substitution. The 1st, 6th, and 8th equations enable us to do this, as they involve respectively 1,4,7; 1,6,9; 6,7,10; that is to say, only two variables (6, 7) besides those wanted. Thus, writing the three equations in the form

$$\left.\begin{array}{llll}(Qx^3 - 2C'x^2y) + Axy^2 & & & = 0 \\ (Cx^3 - 2B'x^2z) & & + Pz^2x & = 0 \\ & - 2A'xyz + Rxy^2 & + Bz^2x & = 0\end{array}\right\}$$

we have

$$\begin{vmatrix} Qx^3 - 2C'x^2y & A & \cdot \\ Cx^3 - 2B'x^2z & \cdot & P \\ - 2A'xyz & R & B \end{vmatrix} = 0 ,$$

or

$$x\{ - 2APA'yz - (ABC + PQR)x^2 + 2ABB'xz + 2PRC'xy\} = 0 ,$$

as it ought to be.

Further, by making in this the interchange

$$\begin{pmatrix} A, & B, & C, & x, & y, & z \\ Q, & R, & P, & \frac{1}{x}, & \frac{1}{y}, & \frac{1}{z} \end{pmatrix}$$

we obtain

$$- 2QCA'x^2 - (ABC + PQR)yz + 2QRB'xy + 2BCC'zx = 0 ,$$

which is another equation of like form (§ 5).

21. Not essentially different from this general method is another process which consists in taking the determinant of the 10th order connected with the above-mentioned ten equations, and transforming it into a determinant of lower order by adding multiples of rows (not columns) so as to increase the number of zero elements. For example, the initial form of the determinant being

$$\begin{vmatrix} Q & \cdot & \cdot & -2C' & \cdot & \cdot & A & \cdot & \cdot & \cdot \\ \cdot & R & \cdot & \cdot & -2A' & \cdot & \cdot & B & \cdot & \cdot \\ \cdot & \cdot & P & \cdot & \cdot & -2B' & \cdot & \cdot & C & \cdot \\ \cdot & A & \cdot & Q & \cdot & \cdot & -2C' & \cdot & \cdot & \cdot \\ \cdot & \cdot & B & \cdot & R & \cdot & \cdot & -2A' & \cdot & \cdot \\ C & \cdot & \cdot & \cdot & \cdot & P & \cdot & \cdot & -2B' & \cdot \\ \cdot & \cdot & \cdot & \cdot & A & \cdot & \cdot & \cdot & Q & -2C' \\ \cdot & \cdot & \cdot & \cdot & \cdot & B & R & \cdot & \cdot & -2A' \\ \cdot & \cdot & \cdot & C & \cdot & \cdot & \cdot & P & \cdot & -2B' \\ \cdot & \cdot & \cdot & -QRB' & -RPC' & -PQA' & -CAA' & -ABB' & -BCC' & ABC+PQR+4A'B'C' \end{vmatrix}$$

we may multiply the 4th, 5th, and 6th rows by R, P, Q respectively, and then subtract from them C, A, B times the 1st, 2nd, 3rd rows respectively. This gives us

$$\begin{vmatrix} QR & 2AA' & \cdot & -2C'R & -AB & \cdot & \cdot \\ \cdot & RP & 2BB' & \cdot & -2A'P & -BC & \cdot \\ 2CC' & \cdot & PQ & -CA & \cdot & -2B'Q & \cdot \\ \cdot & A & \cdot & \cdot & \cdot & Q & -2C' \\ \cdot & \cdot & B & R & \cdot & \cdot & -2A' \\ C & \cdot & \cdot & \cdot & P & \cdot & -2B' \\ -QRB' & -RPC' & -PQA' & -CAA' & -ABB' & -BCC' & ABC+PQR+4A'B'C' \end{vmatrix} .$$

Here the elements $2AA'$, $2BB'$, $2CC'$ can easily be rendered zero, and after them the elements $-QRB'$, $-RPC'$, $-PQA'$ in the same columns. When this has been done the first three columns will resemble the first three columns of the original determinant, so that the simplification made upon that determinant can be repeated, with the effect of reducing the order by as much as before. The result then will be

$$\begin{vmatrix} 2ARB' & 2APA' & \sigma & -2RPC'-4AA'B' \\ \sigma & 2BPC' & 2BQB' & -2PQA'-4BB'C' \\ 2CRC' & \sigma & 2CQA' & -2QRB'-4CC'A' \\ -2CAA'-4RB'C' & -2ABB'-4PC'A' & -2BCC'-4QA'B' & \sigma+16A'B'C' \end{vmatrix}$$

where $\sigma$ stands for $ABC+PQR$. Now this is exactly the form of the eliminant found in § 15—a result not to be surprised at if we consider that the process now gone through is closely allied to the process of eliminating the six variables $x^3$, $y^3$, $z^3$, $x^2y$, $y^2z$, $z^2x$ from the ten equations of § 19.

## Note on the Action of Sodium Mercaptide on Dibromo-Malonic Ether. By Prof. Crum Brown and Robert Fairbairn, B.Sc.

(Read March 18, 1895.)

We examined the action of sodium mercaptide on dibromo-malonic ether in the hope that in this way we might obtain the substance $C(S—C_2H_5)_2(COOC_2H_5)_2$. We found, however, that the reaction takes a different direction, and that what happens is represented by the equation

$$2CBr_2(COOC_2H_5)_2 + 4NaSC_2H_5 = \begin{matrix} C(COOC_2H_5)_2 \\ \ddot{C}(COOC_2H_5)_2 \end{matrix} + 2(C_2H_5)_2S_2 + 4NaBr.$$

The same action took place whether the dibromo-malonic ether was dissolved in ether and gradually added to the sodium mercaptide suspended in ether, or the dibromo-malonic ether and the mercaptide were each dissolved in alcohol and mixed. The product, after the ether or alcohol was distilled off, was distilled *in vacuo.* The fraction passing over between 200° and 250° soon solidified as a yellow crystalline mass. This was very soluble in benzene, and very sparingly so in ligroin; slightly soluble in alcohol, in ether, and in hot water; practically insoluble in cold water. It was easily purified by dissolving it in the smallest possible quantity of benzene, and then adding ligroin. From the mixture the substance separated in large transparent monoclinic crystals. Analysis gave the following results:—

I. 0·1914 grm. substance gave 0·3684 $CO_2$ and 0·1173 $H_2O$
II. 0·1878 „ „ 0·3595 „ „ 0·1071 „

| | I. | II. | Calculated for $C_{14}H_{20}O_8$. |
|---|---|---|---|
| C, . . . . | 52·48 | 52·22 | 53·17 |
| H, . . . . | 6·81 | 6·33 | 6·33 |

The crystals fused at 57°. There could be no doubt that the substance was the "dicarbintetracarbonsäure äthylester" of Conrad and Guthzeit, who give the fusing-point 58° (Liebig's *Annalen*, 214, 76).

In the more volatile product, distilling under ordinary pressure between 150° and 160°, the sulphur was determined:—

0·1812 grm. substance gave 0·6891 $BaSO_4$

| | Found | Calculated for $(C_2H_5)_2S_2$. |
|---|---|---|
| S, . . . . . . | 52·2 | 52·4 |

## A Sketch of Lake-dwelling Research. By Robert Munro, M.A., M.D.

(An Address delivered at the request of the Council, March 4, 1895.)

The comparative security afforded to birds by island-retreats could not fail to have attracted the attention of man from the very dawn of his reasoning faculties, and it is probable that, as soon as he acquired sufficient skill to enable him to cross a creek or a river, he occasionally resorted to such means of protection. From the natural to the artificial island was but a stage of transition which, in the course of time, would be readily bridged over by his progress in mechanical knowledge. To some such sequence in the phenomena of human civilisation must be assigned the origin of those strange habitations known as lake-dwellings. As a means of defence, an island-fort, or village, rudely constructed of timber and situated on the shallow margin of a lake, could offer but little resistance against an attack conducted on the principles of modern warfare. Very different, however, would be the result where the assailers were limited to the appliances in use in pre- and proto-historic times. On this point we are not without some historical evidence, as it is recorded that the dwellers in Lake Prasias successfully defied the military resources of a Persian army; and even, as late as 1566, an attacking party from an English army under Deputy Lord Sydney equally failed to capture a crannog near Omagh, in Ireland. But whatever may have been the primary object of these structures, or the precise circumstances which led to their development, one thing is certain that they continued, for many centuries, to be the characteristic abodes of the early inhabitants of Central Europe wherever the necessary hydrographical conditions were to be found. The remarkable development of the system in Central Europe during the Stone and Bronze Ages seems to have come to a sudden end within prehistoric times, and, indeed, so completely had the custom fallen into desuetude, that scarcely a trace of it has survived in the traditions or annals of those very countries in which lake-dwellings were most abundant. To have rescued so singular a phase of human civilisation from oblivion is one of the greatest triumphs of pre-historic archæology. I propose, therefore, to describe briefly the circumstances which led to the discovery of the sites of so many of these ancient dwellings, and to convey some general idea of the extraordinary wealth of archæological material brought to light by subsequent investigations.

The actual starting-point of lacustrine research may be dated to an incident which took place in Dublin upwards of half a century ago. It appears that early in the spring of 1839 curiosity was roused at the Museum of the Royal Irish Academy by the frequency of the visits of a local dealer offering for sale objects of a miscellaneous character, many of which were of rare antiquarian value. These objects were said to have been found in a peat-bog in the County of Meath, and their assortment in such a place seemed so strange to Dr Petrie that he resolved to go and visit the locality. Accordingly, he and Surgeon Wilde (afterwards Sir W. Wilde) started in search of the mysterious find, and were conducted to the peat-bog of Lagore, near the village of Dunshaughlin. Here, within the boundaries of a drained lake and under a thick covering of peat, was an artificial mound then partially exposed by peat-cutters. This mound had been well-known to bone collectors for upwards of ten years, during which time, it is said, they had dug out and exported to a factory of bone manure in Scotland no less than 150 cart-loads of bones. The mound was of a circular shape, slightly raised above the surrounding plain, and measured 520 feet in circumference. Along its margin were "upright posts of black oak, measuring from 6 to 8 feet in height; these were mortised into beams of a similar material, laid flat upon the marl and sand beneath the bog, and nearly 16 feet below the present surface. The upright posts were held together by connecting cross-beams, and fastened by large iron nails."

That the nature of this mound was correctly interpreted by these eminent archæologists may be gathered from the abstract of Sir William Wilde's paper on the Lagore crannog in the *Proceedings of Irish Academy* for 1840, from which the above extract is taken. It would appear, however, that no great efforts were made to secure the relics for the museum,—a circumstance greatly to be regretted, as this crannog is justly regarded as one of the most important and richest in archæological remains ever found in Ireland. The late Lord Talbot de Malahide, writing in the Archæological Journal for June 1849, says :—"A great portion of these valuable relics became the property of the late Dr Dawson, Dean of St Patrick's, and on his decease were purchased, with the rest of his Irish antiquities, and presented to the Museum of the Royal Irish Academy. Surgeon Wilde also presented to the same institution a valuable collection of the bones found in the same locality. Mr Barnwall, the owner of the soil, still possesses some remnant of this treasure, after having been plundered to a considerable extent by dishonest servants; and those specimens which I possess, representations of some of which are given in illustration of this paper, I owe to the liberality and kindness of the same gentleman."

But, in addition to its wealth of industrial remains, the Lagore crannog possesses a special value in the fact that frequent references have been made to it in the Irish Annals. Hence may be determined with tolerable accuracy the period of its occupancy. Thus, in 843 A.D., we read that Cinaedh "plundered the island of Loch Gabhor and afterwards burned it, so that it was level with the ground." It would appear, however, to have been rebuilt after this catastrophe, as, in the year 933, we find it stated that "the island of Loch Gavor was pulled down by Aulair O'Hivair."

A few months after the discovery at Lagore, an island, "artificially formed of timber and peat," became exposed upon the lowering of the water in Roughan Lake near Dungannon. Another, in similar circumstances, came to light in Lough Gur, County Limerick, from which a vast collection of bones and a number of antiquities were obtained—among the latter being a stone-mould for casting bronze spear-heads. Mr Shirley, in his "Account of the Kingdom of Farney," describes a crannog which had been previously known as "The island Ever MacCooley's house." "The foundations of this ancient house," writes Mr Shirley, "were discovered in the autumn of 1843, 7 feet below the present surface of the earth, in the little island at Lisanisk, and 2 feet below the present water level of the lake a double row of piles were found sunk in the mud; they were formed of young trees from 6 to 12 inches in diameter, with the bark on. The area enclosed by these piles, from which we may judge of the size of the house, was 60 feet in length by 42 feet in breadth."

In 1846 Mr Shirley describes, in the Archæological Journal, two other crannogs—one in Lake Monalty and the other in Loch-na-Glack—which yielded relics of a miscellaneous character, among them being several objects typical of the Bronze Age, as well as others of mediæval and more recent times, such as a gun-barrel and a pistol-lock.

About this time the crannog of Ballinderry near Moate, County Meath, became known, which, judging from the number of objects in the Dublin Museum said to have been found in it, must have been also rich in relics.

But the most important subsequent discoveries were due to the workings of the Commission for the Arterial Drainage and Inland Navigation of Ireland, which brought to light no less than 22 crannogs throughout the Counties of Roscommon, Leitrim, Cavan, and Monaghan. Reports of these crannogs by the engineers of the Board of Works, along with plans, maps, sections, and a large assortment of relics, were deposited at the time in the Museum of the Royal Irish Academy. The objects collected on them would appear to be of the usual heterogeneous kind, but, unfortunately, they were indiscriminately mixed up with the other Irish antiquities in the museum, so that, with the exception of those illustrated in Wilde's Catalogue, and a small collection which found a resting-place in the British Museum, few of them can now be identified.

While these crannog investigations were thus steadily progressing in Ireland, an independent discovery was announced in Switzerland which, not only gave a new significance to the Irish discoveries but, almost immediately, opened up one of the most prolific fields of pre-historic research which has ever come under the cognisance of archæologists. This discovery was indirectly due to the exceptional cold of the winter of 1853–54, which caused the water in Lake Zurich to sink to a lower level than any previously on record —being one foot lower than the celebrated mark on the stone of Stäfa, which preserves the record of a similar phenomenon in 1674. In these circumstances two of the inhabitants of Ober-Meilen whose vineyards came close upon the shore of the lake, began to extend them by enclosing portions of the exposed shore with a stone wall, and filling in the space with mud so as to bring its surface above the ordinary level of the water. In the course of these operations the workmen observed, in the mud taken from the bed of the lake, portions of rotten posts, together with stone axes, flint implements, and other worked objects of horn and bone, which excited their curiosity. Next day Mr Aeppli, the village schoolmaster, heard through his scholars of the curious things turned up in these diggings, and as soon as his day's duties were over he went to see the place. After inspecting some of the objects which the workmen had laid aside, Mr Aeppli thus expressed himself to the interested bystanders :—" *Hier hat die Menschenhand gearbeitet, das sind Wertzeuge und Geräthe, die der Mensch einst gebraucht hat; ihre Form gehört menschlicher Thätigkeit an.*" (*Die Pfahlbauten in den Schweizer-Seen*, p. 8, F. Staub.) He then wrote a short account of what he had seen, and sent it to the Antiquarian Society at Zurich. Within four hours of the dispatch of his epistle three representatives of the Society arrived at Ober-Meilen, among them being the president, Dr Ferdinand Keller.

After careful consideration of the facts, Dr Keller came to the conclusion that the piles had supported a platform upon which huts had been erected, and that, after a long period of occupancy, the entire structures were destroyed by a conflagration.

This important deduction, fanned by the traditional stories of submerged cities long current among the fishing community, spread rapidly among the Swiss people and produced an immediate army of explorers who commenced a vigorous search for similar remains in this and the adjacent lakes. Guided partly by the recollection of previous finds, the significance of which became now apparent, and partly by the knowledge of local fishermen who, from practical experience of disasters to their fishing gear, could at once point to numberless fields of submerged woodwork, the efforts of these pioneer *Lacustreurs* were speedily crowned with the greatest success. In the spring of the same year the famous station, known as the *Steinberg* at Nidau, was discovered, as well as many others in the Lakes of Bienne, Neuchâtel, and Geneva; so that before the report of the Ober-Meilen discovery could be published in the Transactions of the Antiquarian Society of Zurich, Dr Keller had equally interesting materials from other stations to record. This report, which appeared towards the close of 1854 under the title "*Die Keltischen Pfahlbauten in den Schweizerseen,*" at once attracted the attention of archæologists throughout Europe.

Prominent among those who took part in these earlier researches may be mentioned Professors Morlot, Desor, Troyon, and Rütimeyer, all well-known authors of works on lacustrine archæology; Col. Schwab and Mr Müller of Bienne, whose united collections of lake-dwelling antiquities now form the greatest attraction to their native town; MM. Forel of Morges, father and son, whose collection has only recently found an appropriate resting-place in the Museum of Lausanne; Mr Jacob Messikommer, the indefatigable explorer of the famous Pfahlbau at Robenhausen; Mr Albert Jahn of Bern, Dr Uhlmann of Münchenbuchsee, Mr Caspar Löhle of Wangen, Col. Suter of Zofingen, Mr Ullersberger and Dr Lachmann of Ueberlingen, &c., &c. But foremost among them all stood Dr Keller himself who, from time to time, issued systematic reports according as fresh materials came to hand. These reports, 6 of which appeared prior to 1866, the date of the first English edition of Keller's work, were compiled partly from the author's personal observations and partly from data supplied by local explorers in the various districts investigated.

The immediate outcome of the publicity thus given to the existence of a pre-historic lacustrine civilisation in Switzerland was a systematic search for similar remains throughout Europe. But, before discussing such concurrent researches, I will briefly notice one or two subsidiary events which, at a later period, were the means of greatly facilitating the exploration of the Swiss lake-villages. To dredge the bed of a lake with hand-worked appliances in a small boat was a slow process, always expensive and often unproductive. Yet such was the enthusiasm with which this kind of work was carried on, year after year, sometimes at the expense of archæological societies but more frequently by private resources, that there is scarcely a cantonal museum in Switzerland but contains a collection of lacustrine relics secured by these laborious means.

It often happens that antiquarian remains are incidentally brought to light in the course of agricultural operations. Such works are, however, usually confined to small lakes and bogs. The idea of partially lowering the surface of the extensive sheets of water in the Jura valley, comprising the Lakes of Bienne, Neuchâtel, and Morat, was too chimerical to be ever entertained by archæologists. But what was inconceivable and utterly beyond hope from this point of view became, in the interests of agriculture, not only a practical problem, but is now an accomplished fact. Between these three lakes there stretches a vast mossy district, known as the "Gross Moos," through which the combined surplus water of the two latter finds its way to the former. This surplus water again emerges from Lake Bienne and is carried off by the Lower Thielle which, before the "*Correction des Eaux du Jura*," united with the Aar a few miles down the valley. As the surface of these three lakes is nearly on the same level, it is more than probable that, in pre-historic times, their waters formed one united sheet which, in the course of time, became separated into three lakes by the interposition of the sedimentary and peaty deposits now forming the "Gross Moos." Their connecting channels, the Broye and the Upper Thielle, owing to the sluggishness of the flow, became gradually raised by the constant deposition of mud—thus proportionately raising the level of the confined waters, and rendering the surrounding lands more and more liable to submergence. Also, the river Aar, though passing quite in the vicinity of Lake Bienne, went a long way beyond it, and often caused great havoc by flooding the richly cultivated lands which it traversed until it joined the Lower Thielle.

To remedy these defects the Swiss Government entered on the gigantic project of deepening the entire waterway, from the junction of the Lower Thielle with the Aar to the outlet of the Broye, in Lake Morat. The scheme also included the cutting of a new channel for the Aar, by means of which it would be entirely diverted from a considerable extent of its old course and made to debouch into Lake Bienne by a straight and much shorter route. The hydrographical effect of these works, begun in 1868 and only completed a few years ago, was to lower the surface of the lakes from 6 to 8 feet.

In the winter of 1871–72, these operations began to tell on Lake

Bienne, but it was some years later before the others became sensibly affected. When, however, the works were finished, the permanent effect on these lakes, especially on Lake Neuchâtel, was very marked—harbours, jetties, and extensive tracts of shore-land being left high and dry by the subsiding waters. This was the harvest-time of archæology. Many of the sites of the lacustrine villages became dry land, and were visited by crowds of eager searchers; even fishermen forsook their normal avocation, finding it more profitable to fish for pre-historic relics. Government at last interfered with this indiscriminate "howking," and passed a law restricting the privilege of excavating to the authorities of the respective Cantons in whose territories the stations happened to be. Thus, besides a large number of relics collected in the stuff raised up by the dredging machines, the "Correction des Eaux du Jura," as the undertaking was called, greatly facilitated the investigation of the lake-dwellings along the Jura waters.

Another class of works which produced results favourable to lacustrine archæology was the deepening of harbours, the construction of jetties, &c., in the larger lakes, such as those of Zurich, Constance, Geneva, and Annecy. As an example, I may just instance the extensive alterations recently executed in the environments of the town of Zurich which have so entirely changed the aspect of the town in the immediate vicinity of the lake that visitors, whose recollection of it dates farther back than these transformations, would hardly recognise the locality. A splendid bridge now spans the outlet of the Limmat, and on both sides of it are elegant promenades, gardens, and ornamental quays, which occupy what was formerly part of the lake. The filling up of this large area necessitated the use of dredgers, by means of which gravel and mud were raised from the most convenient shallows along the shore and transported as required. Among the localities selected for such operations were the "Grosser Hafner" and the outskirts of Bauschanze. The rich loamy deposits of the "Haumessergrund" at Wollishofen were found to be a suitable soil for the floral and horticultural gardens. All these localities turned out to be the sites of lake-dwellings, and yielded an enormous amount of industrial remains of all ages. Indeed, the collection of relics from Wollishofen, now deposited in the Museum of Zurich, must be considered one of the most important in the whole series of lake-dwelling antiquities.

Sometimes the construction of a railway skirting the shore of a lake was the means of bringing important material to light, as was the case at Concise and Grésine. It was in consequence of information supplied by the engineers of the Mont Cenis and Culoz Railway, while conducting excavations at the latter place in 1856, that the existence of lake-dwellings in Lake Bourget became first known, although no advantage was taken of the discovery for several years afterwards. However, in 1862, at the instigation of Baron Despine and M. Desor, the *Société Savoisienne*, made some preliminary investigation with most encouraging results. Subsequently, and at various times, independent researches were carried out by a number of experienced archæologists, among whom may be mentioned Le Comte Costa de Beauregard, MM. Rabut, Perrin, Revon, Cazalis de Fondouce, and Chantre, all of whom secured more or less extensive collections. It may be interesting to note that through the liberality of Sir Augustus W. Franks, M. Rabut's collection is now in the British Museum.

It would be impossible in this sketch to convey anything like an adequate idea of the successive investigations and discoveries which have been made on the sites of the Swiss lake-dwellings during the last 30 or 40 years, and which have so greatly enriched the principal museums of Europe with lake-dwelling remains. For such details I must refer you to the many special works now published on the subject. I shall therefore proceed to notice briefly the concurrent researches instituted in other European countries, beginning with the valley of the Po.

In 1851 while the harbour of Peschiera, at the south end of Lake Garda, was being deepened, numerous bronze implements, associated with decayed wooden piles, were found in the mud at a particular spot near the north mole of the fortress, which, however, attracted no special attention at the time. The bronze objects were laid aside by the workmen and sold as old metal. A few articles were fortunately sent to the K. K. Antiken Cabinet at Vienna. In 1860 further alterations in this harbour became necessary, and again similar objects were found in the dredged stuff. The works were, on this occasion, conducted under the supervision of M. Lorenz and Col. von Silber, who, in the interests of archæology, collected and preserved the bronze objects. On its being afterwards suggested that they were relics of a palafitte, like those recently discovered in Switzerland, Col. von Silber sent an assortment of them to Dr Keller, with an explanatory statement of the circumstances in which they were found. In this notice Col. von Silber writes :—" Lately, when reading the reports of the Swiss lake-dwellings, I remember the occurrence of a great number of pieces of burnt clay found in the mud. These pieces were of a blackish colour, remarkably thick, and without any definite form. I do not doubt that they have been fragments of the clay covering the huts of the lake-dwellings."

These reported discoveries induced the eminent archæologist, Dr E. Freihorn von Sacken, to visit Peschiera for the purpose of investigating the reputed *Pfahlbauten.* In addition to special researches conducted by himself he had correct details of the results already obtained, and, from these sources, he drew up an admirable report, published in 1864, which clearly established the fact that there had been, in this part of Lake Garda, a true pile-dwelling of the Bronze Age.

Meantime, archæologists were on the look out for palafittes in other parts of the lake. As early as 1861, Cav. Martinati detected piles at a place called Rocca di Garda, near Bardolino, which he considered to be the remains of lake-dwellings ; and Dr Alberti found similar evidence in two localities further south, viz., Il Bor and Porto di Pacengo. But the story of the Lake Garda palafittes becomes now blended with another trail of research which had its origin, as follows, in the western region of the Po valley.

In July 1860 M. Gabriel de Mortillet wrote a letter to Sig. Cornalia, president of the Italian Society of Natural Sciences at Milan, suggesting that remains analogous to those in the Swiss lakes might be found in the lakes of Lombardy. The reading of this letter elicited one or two statements of archæological importance. The vice-president, Sig. Antonio Villa, recalled the fact that a bronze axe-head and some flint arrow-heads had been found in the turf-bog of Bosisio, at a depth of 10 feet; and the president mentioned that he possessed similar weapons which had been found, along with human bones, in the peat-beds of Brenna.

Shortly afterwards the celebrated naturalist, Gastaldi, in an article in *Il Nuovo Cimento,* directed attention to certain antiquities which the turf-cutters were in the habit of finding in the "Torbiera di Mercurago." Subsequently, Gastaldi visited this locality and, along with Professor Moro of Arona, made further researches, the result of which was to leave no doubt that they had here to deal with the remains of a true palafitte. During the next two years Gastaldi's report was considerably enlarged by additional finds at Mercurago; but nothing further of a definite character occurred till the summer of 1863, when Professors Desor and de Mortillet made a visit to Lombardy in search of lake-dwellings.

These distinguished archæologists were joined by Professor Stoppani of Milan, and the result of their labours was the speedy discovery of several settlements in Lake Varese. The investigations were energetically continued by Stoppani after the departure of his friends. Under the auspices of the Italian Society of Natural Sciences he made an exploratory tour of all the lakes in North Italy with encouraging results. In Lake Garda he found traces of palafittes in several places, particularly in the Gulf San Felice di Scavola, where some half-a-dozen sites were detected.

Since these initiatory proceedings the number of sites of lake-dwellings in North Italy has greatly increased, there being now scarcely any of the smaller lakes and turbaries which have not yielded more or fewer remains of this character. They have not yet been found in the larger Lakes of Lombardy, a fact sufficiently accounted for by the physical conditions of these glaciated and rock-cut basins whose rapidly shelving shores afford but a scanty holding for piles.

In addition to the ordinary palafittes there is, in the eastern part of the Po valley, another class of ancient habitations, known as *Terremare*, which are so closely allied to the former that virtually they may be described as land palafittes. They are a later development of the lacustrine system, and, as soon as this relationship was recognised, their subsequent investigation became merged in that of the palafittes. Previous to this, however, the *Terremare* have a specially interesting history of their own which may now appropriately be adverted to.

Shortly after the middle of last century, certain artificial deposits of an earthy substance found scattered over the provinces of Parma, Reggio, and Modena, in the shape of large flattish mounds, became known to agriculturists as possessing great fertilising power—a property which henceforth was turned to advantage by using their contents as manure. To such an extent has this practice been carried that many of these deposits, covering in most instances many acres in extent, have now entirely disappeared. In the course of the excavations various objects of antiquity were found by the workmen, such as Roman coins and tiles, implements of bone, horn, bronze, &c.; the bones of domestic and wild animals, and, occasionally, human bones. But such discoveries failed for a long time to lead to any scientific investigation; and when these mysterious mounds happened to be referred to by the early writers of this century each had a theory of his own to account for them. Thus the celebrated naturalist, Venturi, assigned them partly to the Boii, a Celtic race, who here, according to him, cremated their dead warriors, and ceremoniously threw their weapons and animals taken in war into the burning pyres; and partly to the Romans, who selected these heaps for their dwellings and burial-places. Others supposed them to be the sacred and traditional cemeteries of successive races; and it is a curious fact that many of these mounds are to this day crowned by a modern church or convent, around which the Christians have been in the habit of burying their dead. Nor did the opinion of Gastaldi, published in 1861, throw much light on the matter. Seeing that the Terremare were invariably situated near a running stream, he considered them the heterogeneous debris of different ages—Roman graves, cremations, and funeral feasts,—which had been washed down and re-arranged by floods. But these, and all similar theories, based on the supposition that they were the abodes of the dead, were not in harmony with the domestic character of the pottery and implements turned up. The starting-point of a long series of researches by Professors Pigorini and Strobel, which have now completely cleared up the problem, was the announcement in 1861 that the remains of a palafitte, analogous to those found in lakes and peat-bogs, were to be seen beneath the true terramara-deposits at Castione dei Marchesi. Nearly 100 of these mounds have now been more or less investigated, with the result that there can no longer be any doubt that they are the sites of ancient villages constructed on piles, and fortified by an earthern dyke and a ditch. In their construction one uniform plan was adopted. Having selected a suitable site, always four-sided and orientated but, of course, varying in size according to the requirements of the community, the constructors proceeded to surround it with a ditch, the excavated material being thrown up in the form of a dyke on the inner side. The area thus enclosed was then thickly planted with stakes, the tops of which were brought to a common level, and over them a wooden platform was laid. On this platform cottages made of light timbers and clay were erected. Thus, in a very simple manner, was constructed a fortified village, access to which was secured by one or more wooden bridges spanning the surrounding ditch. The vacant space beneath the common platform became a convenient receptacle for all sorts of refuse, including lost and worn-out objects of industry. When, in the course of time, this space became filled up the *terramaricoli*, in order to avoid the labour of having to remove the debris which would otherwise accumulate around them, adopted the ingenious method of constructing a brand new platform above the former. It seems that a preliminary step to the carrying out of this project was to set fire to the entire village, thus at one *coup* getting clear of all sanitary difficulties as well as of a number of uninvited guests. Having thus started with a clean bill of health, they elevated the dyke to the requisite height, and planted stakes, as formerly, for the support of the new platform and huts—the stakes in this case penetrating only into the accumulated rubbish of the former village. This mode of procedure appears to have been repeated over and over again, until, in the course of ages, the successive deposits accumulated to a height of 15 or 20 feet.

One great objection to this theory, when first propounded by Chierici, was the fact that, except sometimes in the lowest stratum, piles were rarely met with in terramara-deposits. But the difficulty has been satisfactorily accounted for by the readiness with which wood becomes decomposed, when placed in circumstances which render it liable to be alternately dry and wet. Chierici conclusively showed that, although the actual piles had entirely disappeared by decomposition, their former existence could be still demonstrated by the permanence of the holes in which they stood. On this point he relates that, on one occasion, in a space measuring 210 square mètres he counted no less than 124 "buche di pali." Nay, more, some of these holes had become filled up with infiltrated material which subsequently hardened and formed actual casts of the original wood.

The scarcity of fuel in Italy has fostered the habit of utilizing peat-bogs to the fullest extent; and for this purpose the smaller basins, which have become nearly filled up with combustible material, are occasionally completely cleared out. From an archæological point of view no exploration could be more satisfactory than such undertakings, as, with a little care on the part of the workmen, all stray objects are bound to fall into their hands. It so happened that two small basins so treated, viz., Lagozza, in the province of Milan, and Polada, near Desenzano, contained the remains of a lake-dwelling, and both of them have yielded such a valuable series of relics that the entire life-history of their inhabitants can be portrayed.

Perhaps there is no locality in Europe which contains a greater variety of the vestiges of past humanity than the valley of the Po. The unique discoveries made in the cemeteries of Bologna and surrounding district clearly reveal the footsteps of the successive races who inhabited the Circumpadana during, and subsequent to, its occupancy by the lake-dwellers. The lower beds of the terramara-mounds contain the earliest relics of the Bronze Age in Europe, while the upper ones are so intermingled and overlapped with Etruscan, Gaulish, and Roman remains that it is by no means an easy task to decipher their entanglements. But the ethnological problems suggested by these various civilizations I must for the present pass over.

Among the more important of later investigations in North Italy may be mentioned those conducted at Peschiera and Il Mincio (De Stefani), in Lake Fimon (Lioy), in the small Lake of Arquà-Petrarca, near Padua (Cordénons), in the Lakes of Varese and Monate, and in the terramara of Fontanellato, near Parma (Pigorini). On the Isola Virginia, a prettily-wooded island in Lake Varese, there is a museum, erected by Sig. Ponti of Milan, which contains a large assortment of lacustrine relics collected in the neighbourhood.

At the request of the Royal Academy of Sciences of Vienna Professor Von Hochstetter made an investigation of the Lakes of Carinthia and Carniola in search of lake-dwellings, a report of which was published in 1864. But the result was, in the main, of a negative character, and with the exception of the Keutschachersee, the indications of Pfahlbauten observed were too problematical to be of much scientific value. It was confidently expected that traces of them would be found in the Zirknitzersee, as the Chronicler Valvasor (1689) relates that on this lake there was an old bridge the piles of which he himself had seen, but, though carefully explored by Von Hochstetter and Dr Deschmann, nothing of the character suggested was found.

Another locality surmised by Von Hochstetter to contain lake-dwellings, was the great Moor at Laibach. This surmise originated in the fact that, a few years previously, a couple of canoes and a few objects had been dug out of the moss. But it was some twenty years later before the very characteristic examples, whose industrial remains now fill a large room in the Laibach Museum, were discovered and investigated.

The first report of lake-dwellings in the Attersee was published in 1871 by Count Wurmbrand and Mr Simony, and during the following five years further notices appeared, accordingly as fresh materials came to hand.

The most interesting of all the stations in the Austrian lakes was that at See, in the Mondsee, the exploration of which, in 1872 and subsequent years, was due to the indefatigable energy of Dr Much of Vienna, who realized thereby a large and instructive collection of antiquities.

An important station of the Bronze Age was found at the *Rosen Insel*, in the Lake of Starnberg, and investigated under the superintendence of M. von Schab, the Government law officer at Starnberg.

The settlement exposed by peat-cutters in the extensive deposits of peat at the upper end of the Federsee was constructed on principles analogous to those of the artificial islands. Instead of a platform raised on piles, there was a solid basement formed of layers of wood intermingled with clay and other materials. Mr Frank of Schussenried, its explorer, is in possession of a very fine collection of relics, all of the Stone Age. Among them I was shown a consolidated mass of a black-looking material, which, on close inspection, was seen to be composed of grains of wheat. But the curious feature of this relic was that it retained the impression of a finely-woven tissue, evidently that of the sack in which the grain had been kept.

Among the earlier lacustrine discoveries in North Germany were those described by Dr Lisch, curator of the Antiquarian Museum at Schwerin. In 1863 peat-cutters began to find in the Lattmoor, near the town of Wismar, industrial relics which, on being looked into, proved to be remains of lake-dwellings. The lowering of the Persanzigersee, in the same year, disclosed a small island surrounded by a curiously constructed series of wooden compartments, the purpose of which had, for several years, puzzled antiquaries. Ultimately their mystery was explained by the discovery of similar structures in some of the other lakes in North Germany, which were shown to be the basements of lake-dwellings. Another structure of a similar type became exposed, about the same time, in the Aryssee, in consequence of the artificial lowering of its waters. The relics found on this settlement were of so mixed a character as to give rise to a discussion about its age, some archæologists maintaining that it must be dated as far back as the Stone Age. On the other hand, Professor Virchow, who has devoted much attention to lacustrine research, believes that it, as well as many others in North Germany, belongs to a more recent period than that of the lake-dwellings of Switzerland and South Germany. According to him, some of the former have actually been proved to be synchronous with the Burgwälle, which originated with the Slavish people.

Before the construction of the great sea-dykes in Holland, nearly the whole of West Friesland would have been in that hybrid condition described by Pliny, in which it was difficult to say whether it belonged to sea or land (*dubiumque terræ sit, an pars maris*). "Here," says this writer, "a wretched race is found, inhabiting either the more elevated spots of land, or else eminences artificially constructed, and of a height to which they know by experience that the highest tides will never reach. Here they pitch their cabins; and when the waves cover the surrounding country far and wide, like so many mariners on board ship are they," etc.

At the present time this region is richly cultivated, and looks as if it were a dead level, and it is only on close inspection that certain elevations of considerable extent, called *Terpen*, scattered irregularly over the country, can be detected. It is on such elevations that villages and churches are generally built, and, till they accidentally attracted the attention of agriculturists within recent years, nobody seemed to have thought anything about their origin. They are now proved to have been originally constructed as pile-dwellings, precisely similar to the Terremare, and are probably the actual mounds seen and described by Pliny. They might therefore be more appropriately designated as marine-dwellings.

Like the Terremare of Italy, the Terpen are largely excavated on account of their rich ammoniacal deposits, which are used by agriculturists as guano. The industrial remains found in the course of these operations are of a very miscellaneous character, and give a vivid picture of the civilization of their inhabitants from Roman times down to the twelfth century. Among the relics I noticed such objects as the shells of eggs (hen and goose), some of which were unbroken, a flute made of the shank bone of an animal, large casks, canoes, loom weights, toilet combs, iron bridle-bits, beads of glass and amber, Anglo-Saxon, Byzantine and Roman coins, bronze pots, pottery, etc., etc.

Thus the spirit of research, awakened by the discoveries in Switzerland, stimulated archæologists everywhere to be on the *qui vive* for such remains, and gradually led to the accumulation of extensive collections of lake-dwelling remains throughout Central Europe.

In the progress of these continental discoveries Irish archæologists could not fail to be highly interested, seeing that they themselves had already touched the fringe of the subject, and henceforth crannog-hunting was pursued among them with renewed vigour. The annals were now carefully searched for references to crannogs; and many of the localities thus indicated were identified and partly explored. In 1857 Sir W. Wilde published the first part of his well-known catalogue of the antiquities in the Museum of the Royal Irish Academy, in which he gave an excellent account of the crannogs. In it the author states that 46 were known up to date, and predicts that many more would be exposed as the drainage of the country advanced—a prediction which has been amply verified, as every succeeding year has seen an increase to their number. Now the total number of Irish crannogs is upwards of 200. In the following year Dr Keller expatiated on the analogy between the Irish crannogs and the Swiss *Pfahlbauten*, in the Proceedings of the Society of Antiquaries of Zurich; while Troyon and others discussed the subject in the Ulster Journal of Archæology. Meanwhile, reports of further discoveries in various localities throughout the country were published by Sir W. Wilde, Dr Reeves and Mr Ed. Benn.

The next in chronological sequence to contribute to the exploration and literature of Irish crannogs was Mr G. H. Kinahan, whose first paper on the subject appeared in 1863. This was quickly followed by a number of other monographs which greatly helped to disseminate a correct knowledge of their structure and distribution. A few years later Mr Wakeman contributed a series of valuable articles on crannogs—many of which he himself explored—to the Journal of the Royal Archæological Society of Ireland. But to refer to the numerous archæologists who have subsequently taken an interest in the Irish crannogs would greatly exceed the limits now at my disposal. Let me say, however, that this field of research is by no means exhausted in Ireland. At the present time a crannog at Moylarg, Co. Antrim, is in the process of investigation by the Rev. Mr Buick of Cullybacky which, to judge from the two reports already published, promises to be of unusual interest. A complete monograph on a typical Irish crannog is greatly to be desired, as, notwithstanding the numerous explorations already recorded, I cannot recall a single instance that can be so characterized. Even some of the later discoveries, such as the two artificial islands which became temporarily exposed in Lough Mourne, while this basin was being converted into a reservoir to supply Belfast with water, was merely indiscriminately "howked" by all and sundry. When I visited these crannogs in August 1882, some weeks after they became accessible on foot, I was informed that many objects of archæological value had been picked up by the *pro tem.* explorers. One article which, through the courtesy of the finder, I had an opportunity of inspecting, turned out to be of exceptional interest, because of its extreme rarity, viz., an iron-socketed Celt. (See Lake-dwellings of Europe, fig. 125, No. 1.) But such defects must not always be laid at the door of archæologists, as in many cases the opportunity for investigation is past before any knowledge of the discovery finds its way to a competent authority. This, unfortunately, was the case with that remarkable and unique find at Lisnacroghera (the hangman's fort), first described by Mr Wakeman in 1884.

The special interest attached to that find lies in the unmistakable art character (Late Celtic) of a series of military weapons and articles of dress found in a localized part of a peat-bog within the boundaries of a partially-drained lake, consisting of iron swords with bronze sheaths ornamented with Late Celtic patterns, iron spears in long wooden handles with bronze mountings, and other bronze objects, some of which are the mountings for shields. It is difficult to specify precisely all the objects belonging to this group, but it included, at least, four complete sets of armour, all of which must have been dropped within the limits of a very circumscribed area, as they were found on the plot of one man. Subsequent inquiries of the owner of this plot elicited the fact that much decayed woodwork had been encountered in one particular spot, which he well recollected because of the annoyance it gave them in their peat-cutting operations. This wood had, however, been all removed without being seen by any one competent to form an opinion of its structural character. Associated with these Late Celtic relics was an assortment of the usual miscellaneous objects found on crannogs. Hence the theory that there was in this bog a genuine crannog, on which a mortal struggle took place between a troop of cavaliers armed *cap-à-pie*, and the holders of the crannog fort, has some foundation in fact.

The merit of being the first to direct attention to Scottish crannogs belongs to Dr Joseph Robertson, who brought the subject before the Fellows of the Society of Antiquaries of Scotland in a paper read on December 14, 1857. The facts adduced by Dr Robertson consisted chiefly of historic references to island-forts and submerged wooden structures exposed in the course of the drainage of lochs and marshes during the last and early part of this century. But although this kind of evidence conclusively proved the existence of crannogs it gave little information as to their nature and function in the social organisations of the times. The first great discovery which brought them on the field of practical research was made in the Loch of Dowalton, Wigtownshire, about thirty years ago. In order to drain the extensive meadows occupying the western portion of the Dowalton valley, the proprietor, Sir William Maxwell, Bart., conceived and successfully carried out a project of draining the loch by cutting a new outlet through the narrow lip of rock which, at a certain portion of its margin, was the only barrier between its waters and the lower ground beyond. This excavation was completed during the summer of 1863, and, as the waters subsided, a group of five or six artificial islands gradually emerged, like a scene in fairyland, from the bosom of the lake. The antiquarian remains collected on these islands ultimately disclosed a picture of early Scottish civilization hitherto unknown to historians or to archæologists. Sir Herbert Maxwell, to whom the event was especially exciting on account of the bewilderment of the aquatic birds which were in the habit of frequenting the loch, and the tragic fate of its fish, gives the following reminiscence of the circumstances which led to the recognition of the true nature of the islands:—"I remember when Lord Lovaine was taken down to see the drainage operations in 1863, that the islands were just appearing above the subsiding waters. His lordship had, I think, just returned from Switzerland, where he had visited the lake-dwellings there. My father told me that he exclaimed 'Why, here are just the things I have been looking at in the Swiss lakes.'"

In August of that year, Lord Lovaine (now Duke of Northumberland) read a descriptive account of these crannogs at the Newcastle-upon-Tyne meeting of the British Association.

A couple of years later, Dr Stuart, Secretary of the Society of Antiquaries, visited Dowalton, and, owing to the more complete drainage of the loch, was enabled to examine the islands under more favourable conditions. The result of his labours was an elaborate paper to the Society, in which he gave a detailed account of their structure and of the relics found on them; and to which he added all the facts he could glean elsewhere, including some of the contents of the unpublished paper of Dr Robertson.

Among the industrial remains collected on and around these islands were canoes, bronze dishes of Roman origin, bracelets and beads of glass, bronze brooches and other ornaments, crucibles and iron slag, perforated axe-heads and hammers of iron, fragments of Samian ware, querns, hammer stones, a leather shoe stamped with a pattern, etc., etc. From the undoubted Roman element which characterised a considerable number of these relics, the habitable period of the Dowalton lake-dwellings must be relegated back to the early centuries of the christian era.

Since the publication of Dr Stuart's paper, in 1866, little progress was made in the exploration of Scottish crannogs, although traces of them were occasionally noticed throughout the country, till the discovery and excavation of the Lochlee crannog in 1878–79. This was the commencement of a series of explorations, conducted under the auspices of the Ayr and Galloway Archæological Association, which culminated in the excavation of no less than six typical crannogs throughout the counties of Ayr and Wigtown. From a careful consideration of the relics thus collected there can be no ambiguity as to the testimony they afford of the peaceful occupation of their owners. Indeed, among a very large and varied assortment of objects indicating the prosecution of various industries, the war-like element is but feebly represented by a few iron daggers and spear-heads, one or two tips of the cross-bow bolt, and a quantity of so-called sling stones. Among the rarer objects the following may be mentioned:—two spiral finger-rings of gold, and a crucible containing particles of this metal; a gold coin of Saxon origin—supposed to have been originally a forgery, as it was made up of two thin gold plates and a copper core; two cup-marked stones, one of which had the cup surrounded by two concentric circles; a pendant of jet in the form of a cross inscribed in a circle and ornamented with small incised circles; a conical object of rock-crystal highly polished and having some resemblance to the settings on early book-covers; a flat piece of ash wood having both sides ornamented with an incised spiral pattern; and a remarkable fringe-like apparatus made of the long stems of a moss (*Polytrichum commune*).

But the scientific interest of the investigation of crannogs is not confined to the purely archæological remains found among the *débris*. The structural features of the islands and of the houses erected upon them have occasionally been elucidated by the exposure of portions of undisturbed wood-work. On this point the recently excavated crannog of Lochan Dughaill, in Argyllshire, has furnished evidence of an exceptionally interesting character, inasmuch as it conclusively proves that the house was circular and not, as in former instances, rectangular.

Persons who have never taken part in the actual excavation of a crannog can hardly realize the fascination of this kind of work. Should the *débris* turn out to be rich in relics, even the most cynical visitors catch the enthusiasm and watch each turn of the spade with absorbing interest. Every inch of upturned stuff is carefully scanned, and the merest trifles showing workmanship are eagerly picked up. To genuine archæologists the odds and ends of the kitchen-midden, such as food refuse, broken pottery, stray ornaments, worn-out implements and weapons, are veritable treasures.

The earlier evidence adduced in support of the existence of lake-dwellings south of the Scottish border was, in most instances, too fragmentary to be of scientific value. Of this character were the structures in some of the *Meres* of Norfolk and Suffolk, described by Sir Chas. Bunbury, Professor Newton, and the Rev. Harry Jones: also the reputed pile-structure in Cold Ash Common, Berks, noticed by Dr S. Palmer.

In 1866 General Pitt-Rivers communicated to the Anthropological Society of London a paper entitled "A Description of Certain Piles found near London Wall and Southwark, possibly the remains of Pile-Buildings." The author commenced by observing that his attention was directed to the locality by a short paragraph in the *Times* of the 20th Oct., stating that upwards of twenty cart-loads of bones had been dug out of the excavations which were being made for the foundations of a wool warehouse. Here, in a bed of peat, seven to nine feet thick, intervening between the accumulated *débris* of modern London and a bed of gravel, the workmen came upon a number of wooden piles whose tips penetrated into the gravel. Scattered through this peat were numerous articles of human workmanship; also several kitchen-middens containing the nondescript remains of human occupancy. The majority of the relics were of Roman origin, and included coins, tiles, pottery, and articles of dress. In addition to these there were others of ruder construction made of bone and horn, such as knife handles, spear-heads, a couple of bone skates, etc.

In 1870 a circular island, near the shore of the Lake of Llangorse, Wales, was shown by the Rev. Mr Dumbleton to have been constructed after the manner of the stockaded islands or crannogs. In the course of the excavations, remains of a log-flooring, charcoal, food refuse, etc. were turned up, but among them there was no relic of sufficient character to give a clue to the period when the island was constructed or inhabited.

In 1880 the Drainage Commissioners of Holderness found it necessary to deepen some of the drains in that low-lying district, and when this was being done Mr Thomas Boynton's attention was directed to some prepared wood-work and bones of animals found in the stuff thrown out which he regarded as evidence of a lake-dwelling. Such remains were observed at five different localities, two of which have since been more or less explored, with the result that there could be no doubt that they were the sites of human habitations, having some structural resemblance to the fascine lake-dwellings of Switzerland. Some very curious implements made of the articulated ends of the long bones of some large bovine animals, a flint scraper, a stone axe, a bronze spear-head, and a portion of two jet braclets are the chief relics hitherto recorded.

These meagre records comprise nearly all the results of lacustrine research in England previous to the discovery of the Glastonbury lake-village in the spring of 1892. The site of this remarkable settlement occupies some three or four acres of a flat-meadow, within the boundaries of what is supposed, on good grounds, to have been formerly a lake or marsh. Before excavations were begun all that the eye could discern, on the undisturbed surface, were sixty or seventy low mounds huddled in the corner of a field. Only about one-third of these mounds have, as yet, been systematically explored, and, so far, the original surmise that each mound formed the site of a hut resting on a substratum of beams and brush-wood is entirely confirmed. The operations of last summer were almost entirely confined to tracing the village border which has now been uncovered to the extent of 550 feet, or about one-third of its total circumference. According to Mr Bulleid, the discoverer and investigator of the village, the following facts have been established :—

(*a*). That the village was originally surrounded by the water of a shallow mere.

(*b*). That five feet of peat accumulated during the occupation.

(*c*). That a strong palisading of beams, piles, and brush-wood surrounded and protected the village.

(*d*). That the ground work of the village near its margin is artificial in some places for a depth of five feet.

A vast assortment of the heterogeneous *débris* of human occupancy has been gathered on and around the site of the village, including two complete skulls and other bones of man. One of the skulls shows a deep cut as if made by a sword. Many of the industrial relics exhibit the special characteristics of the style of art known as "Late Celtic," the importation of which into Britain preceded, by two or three centuries, the occupation of the island by the Romans; nor does it appear that any of them has been influenced by Roman art. This, indeed, is one of the most interesting features of the Glastonbury find, and hence, should this pre-Roman character be maintained throughout, its antiquities cannot fail to shed an unexpected light on one of the obscurest periods of British history within pre-historic times.

I find it impossible to attempt to give an adequate idea of the number, technique, and purposes of these relics. Suffice it to say that they are made of various materials—stone, flint, bronze, iron, bone, horn, glass, pottery, etc. Among the bronze objects are fibulæ, spiral finger-rings, penannular brooches, and an elegant bowl. Of bone or horn we have needles, pins, handles, long-handled combs, etc. The pottery is often highly ornamented, and some of the devices show unmistakably Late Celtic art. Among the objects of wood are a canoe, the frame-work of a loom, a decorated stave of a bucket, part of the axle of a wheel, with a couple of spokes in their place. On my last visit to Glastonbury I observed a leaden weight shaped like a cheese having the middle of the rim bulging out a little. It weighs 4 oz. 229 grs. This is the only article in the collection of which there may be entertained a suspicion that it has had a Roman origin.

Before concluding this sketch I wish to refer to two recent discoveries which came under my notice last autumn in Bosnia, and which, in my opinion, fall to be classified as pile-structures.

One of these was made near Bihac, on a small island in the bed of the River Una. Here, in a confined area, some 30 paces long by 20 broad, were encountered the stumps of closely-planted stakes in the midst of a large mass of the heterogeneous *débris* of human occupancy, such as broken bones of domestic and wild animals, some cereals, seeds, and fruits; fragments of pottery, spindle whorls, and some half dozen stone moulds for casting bronze celts, together with a large variety of implements, weapons, and ornaments of bronze and iron. Among the relics are some characteristic specimens of La Tène culture, while others belong to Roman and medieval times. From the numerous photographs, plans, and sections, taken during the excavations, there can be little doubt that Herr Radimsky, who conducted the investigation on behalf of the Government, is right in regarding the habitation which stood in this place as a pile-structure. The Una has here a very sluggish course, and for this reason, as well as the existence of some lacustrine deposits in the neighbourhood, it has been surmised that a lake of considerable dimensions formerly occupied this part of the valley. But whether in lake or river the remains in question must be regarded as coming under the category of lake-dwellings. Radimsky informs me that he has good grounds for supposing that four or five other similar stations may be found in this locality.

The other "find" is at a place called Butmir, in the plain of Ilidze, about eight miles to the west of Sarajevo. Some time ago it was observed, while digging the foundations of a dairy, that the soil turned up contained fragments of pottery, flint implements, stone axes, and many other remains of a primitive people. A perpendicular section of a portion of this accumulated *débris*, from six to eight feet in height, showed that the clay, mould, charcoal, and ashes, of which it was chiefly composed, were arranged in strata more or less parallel with here and there wavy undulations. The relic-bed lay immediately over a fine adhesive yellowish clay and occupied an area of several acres. The finding of occasional hollows in this clay suggested to Herr Radimsky, who carried out extensive excavations for the purpose of determining the nature of the settlement, that they might have been the foundations of the huts of the inhabitants. I do not think that this theory offers a consistent explanation of the facts. I may observe that the present fertile plain of Ilidze is composed of the materials brought down by a number of streams and rain-wash from the surrounding hills, and it is highly probable that in earlier times the basin was more or less a lake. Indeed, in winter, portions of it close to this very spot become still submerged, and in the neighbouring ditches the water lies quite stagnant. The yellow clay, on which the culture beds of Butmir repose, was formed by the deposition of a fine sediment in still water, and between it and the beds above there is no clearly-defined line of demarcation, as bits of charcoal were frequently seen imbedded in the clay to a depth of several inches—thus showing that the charcoal and clay were concurrently deposited. The hollows in the underlying clay vary so much in depth, area and outline, that it is absurd to regard them as the foundations of the original huts. Had they presented even some approach to uniformity in outline, however fantastic, the theory might be feasible. I hold they are nothing more than the clay pits from which the inhabitants extracted the clay used in the construction of their huts and in the manufacture of pottery. In support of this view I may point to the immense quantities of broken dishes found in the settlement, and also to the burnt clay castings of the timbers of which the walls of the huts were made, and so largely found in the upper strata of the relic-bed. It seems to me that the entire phenomena, especially the stratification of the materials, can only be explained on the supposition that the huts of the inhabitants stood on platforms supported by piles, and that the refuse containing lost, broken, and worn-out implements had gradually accumulated in the vacant space underneath. This question led to an animated controversy at the Congress of archæologists and anthropologists held at Sarajevo last August; but, of course, the present is not a suitable occasion for entering on the merits of the discussion. A monograph on this "find," illustrated with beautifully coloured plates of the relics, is now in course of publication by the Government, and as soon as it appears the point will be determined.

I have now transported you on the wings of imagination over a wide geographical area, extending from Ireland to Bosnia, and from North Germany to Italy, and shown you that everywhere within its confines the habit of constructing lake- and marsh-dwellings was prevalent in former times. Of the culture and civilization of the inhabitants of these obsolete dwellings, as disclosed by the *technique* of the stray objects left behind them, I have not spoken, as it is a department which lies beyond the scope of this address. Let me, however, just say in a very few words that an analysis of the evidence shows that the lake-dwellers were not a homogeneous people, except where the system became first developed in the early Neolithic Period. There is reason to believe that many of the Stone Age settlements, especially in Switzerland, continued to flourish during the Bronze Age, without any discontinuity of the race, till the sudden introduction of iron into general use, which seems to have been coincident with the appearance of a new people on the scene who subjugated the lake-dwellers and destroyed their villages. The sporadic lake-dwellings, found outside the area of their early development, belong almost exclusively to the Iron Age. Except among a few localized groups these secondary, or as they may be called historic, lake-dwellers had no common bonds of affinity either as regards civilization, race, or language. The vast majority of the Scottish and Irish crannogs flourished in early medieval times, a statement which, according to Virchow, is equally applicable to their analogues in North Germany. The well-known station of La Tène, at the north end of the Lake of Neuchâtel—believed by the earlier explorers to have been a true lake-dwelling—is now shown to have been an oppidum or fort of the Helvetians situated at the outlet of the lake when its waters stood at a lower level than they did in modern times. The remarkable and unique style of art, disclosed by its remains, seems to be identical with that known in Britain as "Late Celtic." It is, indeed, the striking similarity observed between the objects found at Glastonbury and those indicative of La Tène civilization, now found throughout a large portion of Europe, that gives to the English discovery its exceptional importance. It furnishes an ethnological clue which both historians and archæologists would do well to consider. For this reason alone the Glastonbury *trouvaille* bids fair to equal in archæological value anything of the kind previously known.

## On a Human Cyclops. By Alexander Bruce, M.D., F.R.C.P.E. (With Three Plates.)

(Read March 2, 1891.)

While the occurrence of the condition termed Cyclopia cannot be considered as altogether a rare event, the obscurity in which its pathology is still involved makes it desirable that a full description of every case should be put on record. The specimen in my possession was that of a well-formed female embryo which had apparently reached the seventh month. With the exception of the malformation to be specially considered, there was no abnormality either in its external appearances, or in the structure and disposition of any of its viscera.

In the face the first point to attract attention was the remarkable lozenge-shaped single aperture in the middle line above the mouth, which was overhung by a small pendulous projection of skin attached to the forehead immediately above its centre. This projection, which was evidently the only representative of the nose, was moveable, and presented the appearance shown in fig. 1, somewhat like that of a miniature champagne bottle. It was attached by its narrow end and presented a slight dimple at its free extremity. One's first impression on examining the median aperture was that it represented a single eye in the middle line, but a closer inspection revealed the fact that it was evidently formed by the fusion of the appendages of the two eyes, there being on either side a distinct upper and lower eye-lid provided with well-formed eye-lashes. The upper eye-lids were closely fused in the middle line, but between the lower lids was a small fleshy projection which was probably the representative of the caruncula lachrymalis. Above each upper eye-lid there was a distinct eyebrow. The floor of the cavity was somewhat irregular and lined by a highly vascular membrane, through which no indication of an eye could be detected. Below the median eye no indication of a nose was seen. The lips were well formed, except that the small projection in the middle of the upper lip was absent. The ears were also well formed. The forehead was somewhat high and narrow, and sloped somewhat rapidly backwards. When looked at from the side the head appeared unduly elevated in the frontal region, while the part of the occiput which lay immediately behind the lambdoidal suture was inclined almost at a right angle to the posterior or vertical part of the bone.

On removing the scalp the anterior fontanelle was found to be almost entirely closed, while there was a narrow oval gap between the posterior halves of the two parietal bones which was closed by membrane. The dura mater was of normal thickness at the vertex, but the falx cerebri was somewhat defective, presenting less than half the normal depth of that structure. *The tentorium cerebelli, especially in the part which projects in the middle line* above the vermiform lobe of the cerebellum, was considerably thickened, and was somewhat difficult to detach from the brain.

The cerebrum was found imperfectly divided into two hemispheres (fig. 3), while the cerebellum, pons, and medulla presented an almost normal appearance, the only abnormality in the latter being the nearly complete absence of the anterior pyramids, so that the two olivary bodies almost met in the middle line (fig. 2). Similarly the pons seemed smaller than usual. The cerebellum was completely uncovered by the cerebrum (figs. 3, 4). The two corpora quadrigemina could be seen in the small gap between the cerebrum and cerebellum. The cerebrum had the shape represented in figs. 2, 3. When seen from above it somewhat resembled that of a pigeon. The great longitudinal fissure was very shallow, being about a quarter of an inch deep at its posterior part. Two extremely shallow sulci ran from before backwards (as shown in fig. 3) almost parallel to the great longitudinal fissure. When seen from below the cerebral hemispheres were folded posteriorly over the corpora quadrigemina somewhat as a mushroom is over its stalk (fig. 2). They had a rounded edge and an undermined surface. Between the pons and the cerebrum was a large globular mass imperfectly divided into two lateral halves by a shallow groove extending forwards from the upper margin of the pons. There were no crura cerebri, and no distinct interpeduncular space. Above the globular mass was a small spherical projection which appeared to represent an enlarged pituitary body. There was no evidence of olfactory lobes, or of optic nerves. The remaining cranial nerves were all present, and for the most part appeared normal. The third nerve came to the surface in a groove immediately in front of the pons, and about a quarter of an inch on either side of the middle line. Its size was perhaps slightly subnormal. The fourth nerves were extremely fine threads in the normal position. The sixth pair seemed attenuated. The seventh, eighth, ninth, tenth, and eleventh were normal, as was also the twelfth or hypoglossal nerve, except that, owing to the absence of the pyramids, the groove on the inner side of the olivary body in which the hypoglossal nerve appears was very near the middle line. The membranes covering the cerebrum, cerebellum, and medulla were quite normal, but there was a *remarkable thickening of the pia-arachnoid extending round the globular mass described above, and surrounding and partly concealing the infundibulum and pituitary body.* The thickening extended to the posterior surface of the globular mass, and passed forwards until it became continuous with the cerebral mantle. The pineal gland was not found in its normal situation, but I am not prepared to maintain that it was absent, as it is possible that it was accidentally removed with the tentorium cerebelli.

*The vascular system of the brain.*—The arteries of supply, viz., the vertebrals, basilar, and carotids appeared small in size but otherwise normal. The branches of these arose in their normal positions. The anterior cerebrals passed forwards between the enlarged infundibulum and the cerebral envelope, and then ascended in the shallow median fissure to the vertex of the cerebrum, while the middle cerebral passed outwards in the substance of the thickened membranes to the concave under surface of the lateral portion of the hemisphere. A small posterior-communicating branch united the middle and posterior cerebrals on either side.

There was no trace of the pulvinar, corpora geniculata, or brachia to the corpora quadrigemina. The corpora quadrigemina were quite distinct and moderately well developed.

The brain was carefully hardened in toto for two months in Müller's fluid, and then cut into transverse sections about $\frac{1}{2}$ inch thick.

In the *interior of the brain* there was no indication of the presence of the corpus callosum, fornix, or septa lucida, and there was, consequently, no differentiation of the lateral from the third ventricles. At the bottom of the shallow longitudinal fissure the two hemispheres were connected by white brain substance about one-quarter of an inch in thickness, but this did not present any of the characters of the corpus callosum. A peculiar structure composed of cerebral substance, whose relationships were not at first apparent, was found in the interior of the single ventricular cavity. It extended almost to the anterior extremity of this cavity, and appeared to be formed of an invagination of the posterior wall of the cerebral mantle. Fig. 5 shows the appearance of this structure as seen from its anterior aspect. It had the shape of a horse-shoe, the ends pointing downwards and being attached to the cerebral substance at a level slightly below that at which the section was made. The anterior margin was rounded, and on its surface numerous vessels ramified. Fig. 6 shows the appearance of this structure as seen from behind. The dark space v. represents that part of the ventricular system continued backwards underneath the cavity of the horse-shoe-shaped structure, which must, I think, be considered as the equivalent of the third ventricle, while the narrow slit l.v. seen on the left side points to the hinder extremity of the part of the ventricular system superior to the arched structure which may be regarded as the lateral ventricle.

Between the dark slits marked v. and l.v. are two curved bands or laminæ of white matter, convex backwards. The anterior or narrower of these bands is separated from the posterior or broader band by a narrow convex space. This interval was occupied by a little vascular connective tissue. Both laminæ at their outer extremities passed into the substance of the hemispheres. Examination of a series of microscopic sections indicated that the upper and lower laminæ became continuous round the anterior margin of the horse-shoe-shaped structure depicted in fig. 5, while the inferior laminæ at its extremities became fused with the rudimentary basal ganglia. The relationships of this peculiar structure seemed to me to indicate that it had been formed in the same way as the velum interpositum of the normal brain, namely, by an ingrowth of vascular connective tissue of the pia mater carrying before it a duplicature of the posterior cerebral wall, but that, contrary to what happens in the normal brain, this growth was too feeble to reach the anterior wall of the ventricle and to reduce the brain substance covering it to a single layer of cubical epithelium.

Below the cavity v. the representatives of the two optic thalami were fused together over the mesial plane, except where a small slit remained to represent the infundibuliform portion of the third ventricle. The pituitary body (p.b.) was found surrounded with thickened membranes (fig. 6).

Fig. 7 was drawn from a section made through the optic thalami immediately behind the infundibulum about the anterior part of the large globular mass described above, and shows the fusion of these structures with a narrow slit between them. A series of sections made backwards showed that this narrow slit-like ventricle terminated below the commencement of, and was not in communication with, the aqueduct of Sylvius. Above, it joined the large ventricular cavity of the hemisphere. It was, therefore, evidently, a much narrowed third ventricle. The manner in which the cerebral mantle folds over the optic thalami is well seen in this diagram. In order to ascertain the condition of the thickened membranes round the fused optic thalami, etc., and the condition of the optic nerve, which had not as yet been detected with the naked eye, a series of microscopic sections was made through the globular body in front of the pons varolii. Fig. 8 is drawn from a section through the hinder portion of the optic thalamus, and shows the great thickness and vascularity of the membranes. The two thalami are completely fused together, with the exception of two small openings in the mesial plane, of which the anterior is the extreme lower extremity of the infundibulum, and the posterior the upper portion of the aqueduct of Sylvius. Beneath the infundibulum are seen nerve fibres decussating with each other—few in number, but which, from their position, could only represent imperfectly-developed optic tracts.

Staining by Weigert's hæmatoxylin method demonstrated the existence of medullation of these fibres.

Sections made immediately below this level showed extremely well the remarkable thickening of the membranes (fig. 9). Sections through the nuclei and roots of origin of the third nerve demonstrated that, to all appearance, these were normal. The fillet and red nucleus were both present, but the crustal portion of the crura cerebri was quite absent, and with it, therefore, all indication of the pyramidal tracts.

The contents of the median eye were removed, and, after hardening, cut into transverse vertical sections. These revealed the presence of two rudimentary eyes in which the lens, the retina, and the choroidal pigment layer could be more or less distinctly made out. The appearance is shown in Pl. III. The retina presented a most remarkable appearance. In the mesial plane it was found in an extraordinarily convoluted condition, so that it had assumed in places an almost tubular structure. This is indicated in the middle of the figure. There was no doubt, however, that this was really composed of retinal elements and not of a Schneiderian membrane, as was suggested to me, for every here and there a perfectly distinct retinal structure could be made out. There can be little doubt, I think, that the optic vesicles in their forward growth had, as it were, lost their way, and grown partly along the mesial plane, and partly laterally towards the two eyes. The two eyes were embedded in a somewhat fatty connective tissue, in which could be found indications of the lachrymal gland. Internal to the lenses on either side was a small nodule of cartilage whose origin was somewhat doubtful, but which probably represented an attempt to form some of the bones of the nose. There were large hæmorrhages in various positions external to the choroidal pigment, and amongst the convoluted folds of the retina was a granular substance, in all probability formed of vitreous humour.

Sections at posterior levels to the above demonstrated the existence of several of the ocular muscles—the external, inferior, and superior recti being quite easily recognised.

The optic nerve was single, and presented on transverse sections a convoluted structure containing elements closely resembling those seen in the retina.

## DESCRIPTION OF THE SKULL.

I. *Base of the cranial cavity* (fig. 10).—The presphenoid bone and the ethmoid were completely absent, but in the middle line, in their place, was a foramen bounded (*a*) laterally by the two somewhat deformed and elongated lesser wings of the sphenoid bone, (*b*) anteriorly by the frontal bone, and (*c*) posteriorly by the sella turcica of the sphenoid bone. The post-sphenoid bone and the greater wings were almost normal. The sutures in front of the two petrous portions of the temporal bone formed almost a straight line across the skull. Apparently this was the result of the greater wings of the sphenoid being abnormally approximated to each other. The position of the sutures of the remaining bones entering into the floor of the cranial cavity is, as shown by fig. 10, quite normal, with the exception of that between the two frontal bones which was absent posteriorly. There was a minute foramen in the floor of the sella turcica, which probably represented the remains of Rathke's canal.

II. *The anterior aspect of the bones of the face* (fig. 11).—In the place of the two orbits and nasal bones was a large median cavity, nearly elliptical in shape, measuring transversly $\frac{7}{8}$ in., and vertically about $\frac{5}{8}$ in. This was bounded above by the frontal bone, and laterally by the malar and a small portion of the superior maxillary bone. There was no trace of the ethmoid, vomer, nasal, or lachrymal bones, or of the nasal process of the superior maxillary bones. The median cavity between the lesser wings of the sphenoid and the two sphenoidal fissures are seen in the diagram. On the floor of the cavity there projected backwards from the superior maxillæ, over the distance of about half an inch, a flat plate of bone $\frac{1}{4}$ in. broad. This apparently represented the orbital plates of the superior maxillary bones which had become fused in the middle line owing to the absence of the bony structures which normally intervene. The frontal bones presented a boss-like projection about $\frac{1}{2}$ in. above the orbit, from which the lateral parts of the bone sloped somewhat rapidly backwards and outwards. Above this projection a slight trace of median suture was seen. On the internal aspect of the bone the orbital plates were not horizontal, but sloped gradually upwards into the more vertical portion of the bone. In the superior maxillæ the two infra-orbital foramina were within $\frac{5}{16}$ in. of each other. There were no pre-maxillary bones. The malar bones seemed normal in form and in their articulations, but their anterior extremities were abnormally approximated, owing to the defective development of the superior maxillæ. The lower jaw was normal in form, but projected about $\frac{1}{4}$ in. beyond the superior maxilla, apparently owing to the defective development of the latter. In the sphenoid bones the greater wings had their outer surfaces directed a little more forward than normally.

The foramen cæcum was situated nearly $\frac{1}{2}$ in. from the hinder margin of the frontal bone. At this point the defective falx cerebri commenced as a single layer. Below this point the falx had split into two layers directed outwards towards the anterior lips of the lesser wings of the sphenoid, including between them a small triangular space, into which a small portion of the anterior tips of the cerebrum projected.

III. *Description of the base of the skull.*—The roof of the mouth was formed by the two palatal processes of the superior maxillary bones, which united in the normal way in the middle line. The palate bones presented a peculiar appearance. Instead of being horizontal they were directed obliquely upwards and backwards, and apparently articulated directly with the basi-sphenoid bone. The nasal cavity was thus completely absent. The space included between the orbital plates of the superior maxillæ above and their palatal processes below, between the anterior portion of the superior maxillæ in front and the palate bones behind, was occupied by a large sinus, which was probably the equivalent of the two antra of Highmore. The space between the two palate bones and the basi-sphenoid and occipital bones was nearly hemispherical, with a diameter of about $\frac{3}{8}$ in.

In this case we have to deal with a malformation in which the following facts have to be accounted for:—

1. The fusion of the two eye-lids with each other so as to form a single oval aperture.
2. The formation of two rudimentary eyes in which the lenses are separate, while the retina and optic nerve are single.
3. The absence of certain bones of the face and skull which are normally present in the mesial plane.
4. The absence, or rather displacement with imperfect development, of the nose.
5. Certain defects of development of the cerebral hemispheres and crura cerebri, described above in detail.

It is obvious that the malformation must have been produced at a very early stage of intra-uterine life before the union of the fronto-nasal with the maxillary processes. Some cause must have been operative which has pushed the fronto-nasal process upwards, approximated the two eyes and ocular apertures, fused the two optic vesicles, and prevented the development of the pre-sphenoid bone and the bones of the nasal septum. In our ignorance of the order in which these events took place there must be always a difficulty in determining the true cause. Was it, for instance, a primary compression of the two eyes towards the middle line, and an upward displacement of the fronto-nasal process, or was it a primary compression, with fusion, of the two optic vesicles, and a consequent directly forward growth of these structures? It will be obvious that any cause which directs the optic vesicles forward will prevent the development of those cartilaginous and ultimately bony constituents of the skull which lie between the two optic foramina and in the nasal septum. These are the pre-maxillary bone, the nasal process of the superior maxillary, the nasal bones, the lachrymals, the ethmoid and vomer—in fact, the very structures which are absent from my case. Misformed growth of the optic vesicles will also displace the fronto-nasal process upwards, and approximate or fuse the two eyes and ocular apertures. An intra-cranial cause, therefore, capable of fusing the two optic vesicles together, would appear to be able to produce cyclopia. On the other hand, a similar condition might be produced by any external pressure capable of approximating the two eye-balls. An examination of the cases recorded in Dareste's work on "The Production of Monstrosities" shows that pressure of the head-fold of the amnion may produce this effect. Kundrat has also shown in "Arhinencephalie" that the result of this will be to press the two optic vesicles more closely together, and to drive the cerebro-spinal fluid in a backward direction—for the most part into the diverticulum connected with the pineal gland, which thus becomes dilated into a large cyst. According to Kundrat, there is almost invariably also a dilatation of the cavities of the forebrain. There is also sometimes a dropsical condition of the third ventricle, with a more or less complete atrophy of the optic thalami, and a similarly dilated condition of the primary optic vesicle, which remains single; or, more rarely, the two optic thalami may be fused into one. According to Kundrat, Dr. BRUCE ON A HUMAN CYCLOPS — Plate I.

Fig. 1.

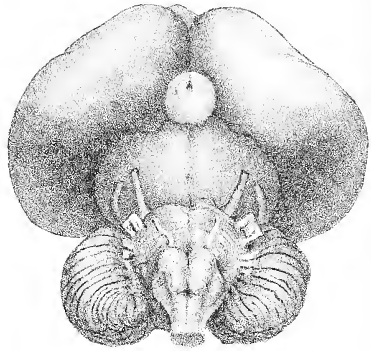


Fig. 2.

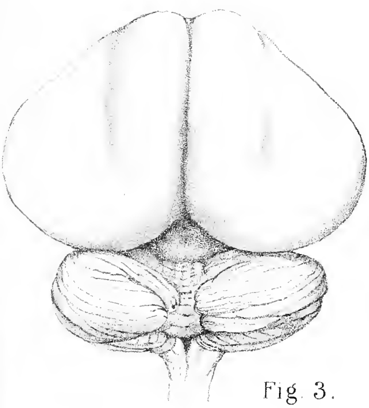


Fig. 3.

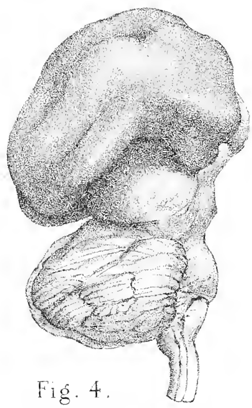


Fig. 4.

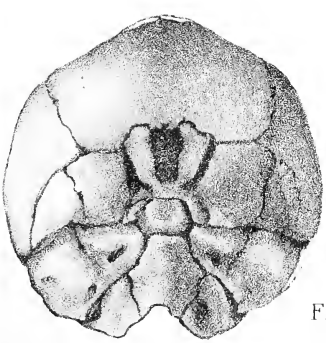


Fig 10.

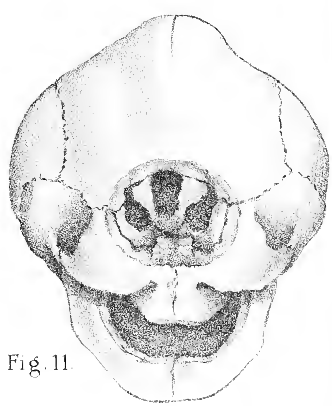


Fig. 11.

McFarlane & Erskine, Lithrs. Edinr.

Dr. BRUCE ON A HUMAN CYCLOPS. — Plate II.

Fig. 5.

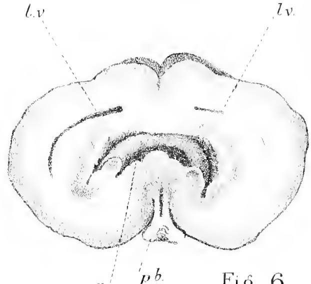



Fig. 6.

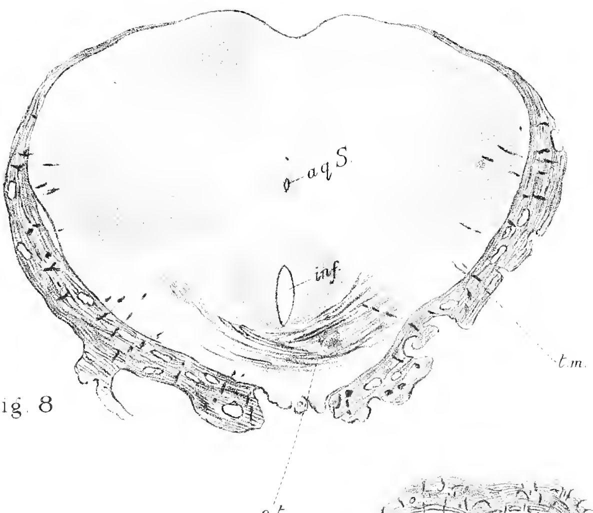



Fig. 8

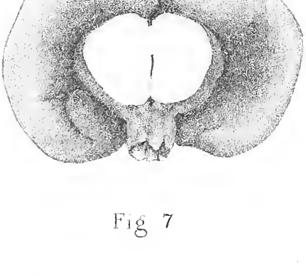


Fig. 7

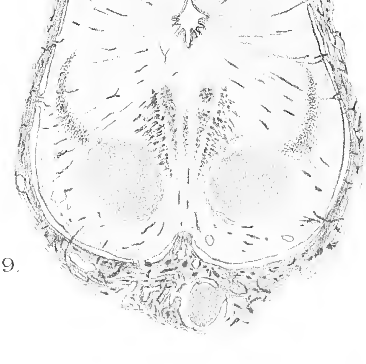


Fig. 9.

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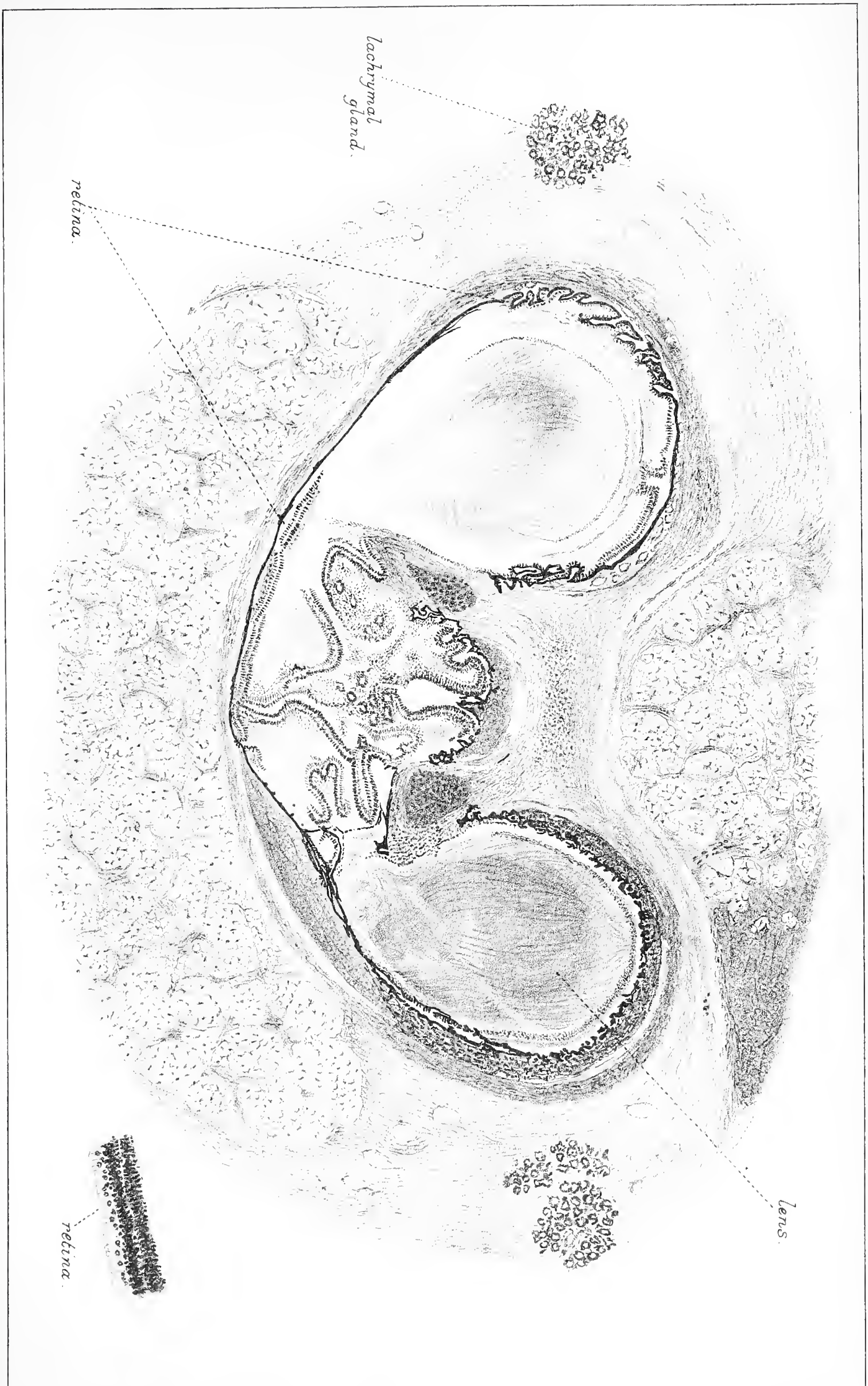
DR BRUCE ON A HUMAN CYCLOPS. — PLATE III.




McFarlane & Erskine, Lithrs Edinr

this condition is almost invariably due to pressure of the head-fold of the amnion. It is obvious that a dilatation of the third ventricle, occurring at an early stage, may drive forward its floor so that the optic vesicles form a single fused cavity, and so cause cyclopia. It is probable that this is a very frequent cause, but it is not the one operative in my case, where there was neither pineal cyst nor dilated ventricle.

The two optic thalami, on the other hand, are all but completely fused together, as are also the two crura cerebri. This fusion is associated with an excessive development of the lepto-meninges round them, by a fusion of these with thickened pachy-meninges at the junction of falx and tentorium, and also by an almost complete disappearance of the optic tracts. The causation of this chronic meningitis is not evident, but its occurrence is apparently so uncommon in the described cases of cyclopia that it cannot be looked upon as a change secondary to the malformation. However produced, it is certainly capable of causing the fusion of the two thalami, and of altering the optic vesicles from an antero-lateral to a directly-forward course. I would submit, then, as a possible separate, or at least contributing, cause of cyclopia, a limited pachy- and lepto-meningitis. The occurrence of this meningitis may serve to explain the restrained ingrowth of pia mater to form the velum interpositum, and therefore the imperfect atrophy of the brain substance within the cerebral vesicle.

The remaining changes in the crura, pons, medulla, and cord were mainly due to the absence from them of the descending tracts from the cerebrum.

# On M. Dubois' Description of remains recently found in Java, named by him Pithecanthropus erectus. With Remarks on so-called Transitional Forms between Apes and Man. By Professor Sir William Turner, F.R.S.

(Read February 4, 1895.)

Since the time when naturalists were led, by the publication of Charles Darwin's far-famed work on the *Origin of Species by Natural Selection*, to consider that Man might have been derived through a process of evolution from lower forms of animal life, attention has repeatedly been called to remains, more or less fossilised, which were thought to be transitional forms between the lower animals and Man.

One of the most remarkable specimens studied from this point of view was the well-known Neanderthal skull, discovered in 1857, along with some bones of the limbs and ribs, in a limestone cave in the Neander Valley. Unfortunately, it consisted only of the calvaria or skull-cap, which was characterised by the great prominence of the glabella and supra-orbital ridges, the flattening of the vertex, the slope upwards and forwards of the occipital squama from the protuberance of that bone, and the long, straight squamosal suture, in all of which it approximated to the configuration of the crania of anthropoid apes. On the other hand, its estimated capacity of 1230 cubic centimetres and its glabello-occipital length of 200 mm. much exceeded the corresponding measurements in anthropoid apes, and approximated it to many aboriginal Australian crania: whilst, in its breadth of 144 mm., it considerably exceeded the transverse diameter of the cranium of the Australian savage. Its length-breadth index is 72. By some observers it was regarded as transitional between man and apes, and Professor King of Galway designated it *Homo Neanderthalensis*. Professor Huxley made a careful analysis of its characters in *Man's Place in Nature*,* and whilst speaking of it as the most pithecoid of human crania up to that time discovered, he showed its affinities to the skulls of some

* London, 1863. Also supplementary paper in *Nat. Hist. Rev.*, July 1864.

of the Australian aborigines, which are flattened on the vertex, and to crania belonging to the people of Denmark during the Stone period. He regarded it as a human skull forming the lowest term of a series leading gradually upwards to the best developed human crania, and stated that the Neanderthal man was in no sense intermediate between man and apes.

In a paper which I read to this Society thirty-one years ago,* I compared the Neanderthal skull with a number of specimens both of savage and British crania in the Anatomical Museum of the University. I showed that the Neanderthal characters are closely paralleled in skulls of existing savage races, and even in occasional specimens of modern European crania; and that the large transverse parietal diameter compensated for the brain space lost by the retreating forehead and flattened occiput. Shortly after the publication of this paper, Dr—now Sir Arthur—Mitchell presented me with a calvaria found in Aberdeen, whilst digging the foundations of Gordon's Hospital, which is built on the site of the Blackfriars Monastery with which a burial-ground had been connected. This specimen confirmed, in a very striking manner, the demonstration which I had previously given.† The conclusion above arrived at is now so generally accepted, that anthropologists not unfrequently refer to specimens of the crania of both savage and civilised races, which they are examining, as possessing Neanderthaloid characters.

Subsequent to the discovery of the Neanderthal skull, other crania have been obtained which exhibit approximately similar characters.

Two of the most remarkable of these were procured along with other bones of the skeleton, in 1886, in a terrace at the mouth of a cave at Spy, Belgium, and have been described by MM. Fraipont and Lohest,‡ who regard them as belonging to the same race as the man of the Neander Valley. Associated with these skeletons were bones of existing mammals, and of the extinct *Rhinoceros tichorinus* and mammoth, also examples of worked flints. They came to the conclusion that whilst the men of Spy had possessed a number of pithecoid characters to a greater extent than in any other human

* Abstract in *Proc. Roy. Soc.*, Edinburgh, January 18, 1864, and *in extenso* in *Quart. Jour. of Sc.*, April 1864.

† The calvaria was described and figured in the *Quart. Jour. of Sc.*, October 1864.

‡ *Recherches ethnographiques sur les ossements humains*, &c. Gand, 1887.

race, yet that they still appeared to be human, and that between them and an undoubted anthropoid ape there was an abyss; though the interval was not quite so great as that between the men of Spy and the fossil Dryopithecus of the Middle Miocene period.

MM. Fraipont and Lohest attached considerable importance to the form and extent of the antero-posterior curvature of the condyles of the femur, and to modifications in the curvatures of the articular surfaces of the head of the tibia, so as to make them conform to the large articular surfaces of the femoral condyles. They considered that with such an extent of curvature backwards of the femoral condyles, the erect human position would not have been possible, and that the trunk had been projected forwards. They are inclined to think that the attitude of these men, when standing, partook more of that of anthropoid apes, and was therefore more pithecoid than human.

Dr Collignon had previously called attention,* in his description of the human skeletons found in 1869 at Bollwiller, in the Department of the Upper Rhine, to the backward slope (retroversion) of the head of the tibia, which gave the articular surface an oblique direction from above downwards, and from before backwards. He regarded it as a character akin to that found in the gorilla, associated with demi-flexion of the leg, and rendering the vertical attitude difficult, so that the body was less erect during progression than in existing men.

In a review † of MM. Fraipont and Lohest's memoir, Dr Collignon expressed the opinion that the characters of the head of the tibia in the Bollwiller skeletons, existed in a higher degree in the tibiæ of the men of Spy. In the following year M. Fraipont published an account ‡ of a fresh examination of the tibiæ obtained at Spy, and corroborated Dr Collignon's opinion. He stated that the incurvation of the head upon the body of the tibia was very accentuated in these skeletons, and he considers that he is, as a result of this additional inquiry, still more justified in concluding that the men of Spy had an attitude less vertical than existing man, and

* *Revue d'Anthropologie,* 1880, vol. iii. pp. 406, 412.

† *Revue d'Anthropologie,* 1887, 3rd series, vol. ii. p. 742.

‡ *Ibid.*, March 1888, vol. iii. p. 145.

that Man has acquired a more erect position since the Quaternary period.

In a memoir on the skeleton of a man referred to the Quaternary period, which was exposed in October 1888, along with flint flakes and worked portions of reindeers' bones and horns, at Reymonden, in the commune of Chancelade, in the Dordogne, Professor Testut described * a broken tibia, the upper end of which had the same backward direction as in the skeletons from Bollwiller and Spy. He regarded it as an ape-like character, indicating that in the standing position the knees projected more prominently forwards than in existing races. The skull was dolichocephalic, the length-breadth index being 72. The cranial capacity, taken by Broca's method, was 1730 c.c., which is greatly in excess of the mean of modern European men.

In arriving at the conclusion as to the signification of the form and direction of the femoral condyles and the superior articular end of the tibia, these observers had not sufficiently taken into consideration the influence which position or attitude would exercise in modifying the bones of the limbs, and the effects which would be produced by occupation, habit, and muscular action on the bones, when in the plastic stage of growth. In the memoir which I published in 1886 on Human Skeletons, in the Reports of H.M.S. *Challenger*,† I called attention to the squatting attitude assumed by so many savage races, as a factor to be considered in determining the shape of the pelvis and the curvature of the lumbar spine. I also pointed out the influence which might be exercised on the form and extent of the areas for muscular attachment on the scapula, in those races of men who are in the habit of climbing trees in search of food, or for other objects.

The influence of the squatting posture in modifying the form of the external condylar surface of the tibia, and in extending the articular areas of the tibio-astragalar joint in savage races, has now been worked out in detail by Professor Arthur Thomson, of Oxford.‡

* *Bulletin de la Soc. d'Anthropologie de Lyon*, t. viii., 1889. Lyon, 1889.

† Zoology, Challenger Expedition, part xlvii., 1886, pp. 58, 77, 88. See also my Lecture on Variability in the Skeleton in different Races of Men, in *Journ. of Anat. and Phys.*, April 1887, p. 473, vol. xxi.

‡ *Journ. of Anat. and Phys.*, July 1889, vol. xxiii. p. 616, and additional paper in the same *Journal*, Jan. 1890.

Professor Manouvrier published in the following year an elaborate paper on retroversion of the head of the tibia, and on the attitude of man in the Quaternary period.* He examined several hundred tibiæ of neolithic men, modern Parisians and savage races, and arrived at the conclusion that, in a notable proportion of these, tibiæ occurred in which the head was as strongly inclined backwards as in the men of Spy, and in some instances, as in the tibiæ of the Indians of California, even more so; and yet these people assume, without a shadow of doubt, a vertical attitude when standing.

Professor Havelock Charles has studied the bones of the lower limbs in natives of the Punjab,† who habitually assume the squatting attitude. He confirms Professor Thomson's observations on the articular surface of the head of the tibia, and the additional facets at the tibio-astragalar joint. He also figures the retroversion of the head of the tibia, and describes modifications in the upper and lower articular ends of the femur, and in the acetabulum, all of which he associates with the squatting position.

It is obvious, therefore, that position and habit materially modify the forms of the bones, and that characters which MM. Collignon and Fraipont thought to be indicative of an inability to attain, in the full sense, the erect attitude, were due to the customary position of squatting, which both ancient and modern savages assumed when at rest. We have no evidence, therefore, that Quaternary man was not as capable of raising his body to the erect attitude as the men of the present day; and Professor Testut's observations further show that a tibia with a retroverted head may be associated with a skull of unusually high internal capacity.

Thus the retroversion of the head of the tibia, to which the above observers attached so much importance, is of no value as a proof of the existence of a transitional form between man and apes.

A few months ago, M. Eugène Dubois, surgeon in the army of the Indian Netherlands, published‡ a memoir descriptive of some bones recently found in Java. From the title of his work, "*Pi-*

* *Mémoires de la Société d'Anthropologie de Paris,* 2nd series, t. iv., 1890.

† *Journ. of Anat. and Phys.*, Oct. 1893, April 1894, vol. xxviii.

‡ Batavia, 1894.

*thecanthropus erectus*, eine menschenähnliche uebergangsform," it is obvious that he considers he has established the existence of a link connecting together apes and man. He names this supposed link *Pithecanthropus erectus;* and as he believes it to differ in characters from man on the one hand, and apes on the other, he proposes to found a new family in the Primates, intermediate between *Simiidae* and *Hominidae*, to which he gives the name *Pithecanthropidae.* He defines the characters of this family as follows :—

Brain case, absolutely and in relation to the size of the body much more spacious than in *Simiidae*, but less spacious than in *Hominidae:* contents of the cranial cavity about two-thirds of the average contents of that of man ; the slope forward of the occipital bone below the protuberance and superior curved line much stronger than in the *Simiidae.* Teeth, although retrogressing, yet of the type of the *Simiidae.* Femur, in its dimensions, like the human, and constructed for progression in the erect attitude.

He believes that the successive stages of evolution up to man are represented by the following forms :—*Protohylobates*, a primitive form of Hylobates; *Anthropopithecus sivalensis*, a form of Chimpanzee of the later Miocene or older Pliocene Period ; *Pithecanthropus erectus*, a late Pliocene or early Pleistocene mammal ; lastly, *Homo sapiens.*

The specimens on which these conclusions are based are a calvaria or skull-cap ; an upper third molar tooth, which he says is the right ; a left femur. They were procured in the neighbourhood of Trinil, in the district of Ngawi, in the Residency of Madiun, on the left bank of the river Bengawan in Java. In September 1891 the molar tooth was got about 1 metre below the dry season mark of the river. A month later, and 1 metre distant from the spot where the tooth was lying, and on the same level, the calvaria was found. In August of the following year, also during the dry season, and 15 metres (nearly 49 feet) higher up the stream, and on the same level, the left femur was excavated. During the dry season of 1893 search was made for other remains, but without result. The bones were embedded in the bank of the river from 12–15 metres below the plain in which the river had excavated its bed. The bank was formed of Pleistocene alluvial deposits, consisting largely of re-arranged andresite tuffs—the loose ejectamenta of volcanic eruptions.

*Skull-Cap.*

This consists of the vault of the cranium from the glabella and supra-orbital arches in front to two finger-breadths below the occipital protuberance (inion) and superior curved line. It is a long ovoid, 185 mm. in glabello-occipital length; 130 mm. in its greatest transverse breadth; 90 mm. in breadth immediately behind the orbits, a dimension which, the author says, would probably have been 4 mm. greater in the unbroken skull. The highest point of the vault of the skull was in the parietal region, and was 62 mm. above a sagittal line drawn horizontally backwards from the glabella to the inferior curved line of the occiput. The relation of length to breadth was as 100 to 70, so that the skull was dolichocephalic. The supra-orbital ridges and glabella had great prominence, and the frontal sinuses were well developed. The greatest sagittal depth of a frontal sinus was 24 mm. The sagittal diameter of the cranial cavity was 155 mm. The thickness of the occipital bone a little below the inferior curved line was 4·5 mm. The frontal bone was slightly keeled in the line of the obliterated frontal suture, and the other sutures of the cranial vault were obliterated. The general surface of the outer table of the skull was smooth, and there was an absence of bony ridges. The vault of the skull had an arch much below the European human skull, but higher than that of anthropoid apes. The supra-inial part of the occipital bone sloped upwards and forwards from the inion, as in the Neanderthal skull, whilst the infra-inial part, to which the muscles of the back of the neck were attached, sloped downwards and forwards to where the foramen magnum had been, though the actual position of this hole cannot be stated with certainty. The forward slope of the nuchal part of the occiput was, without doubt, in relation to the curve of the encephalon and the greater volume of the cerebrum in relation to the cerebellum, which one associates with the erect attitude. From the obliterated condition of the sutures the skull was obviously that of a person not below middle life. Dubois thinks, from the absence of ridges and from the superior temporal lines on the opposite sides of the skull being quite independent, that the cranium must have been that of a female. In the projection of the glabella and supra-orbital ridges the skull had, however, characters which one is accustomed to regard as masculine.

The cerebral cavity was, for the most part, filled with a stony mass, so that the capacity of the skull-cap could not be directly ascertained. Even if the calvaria had been free from its stony contents, the absence of the base of the skull would have made it impossible to obtain a direct determination of the entire cranial capacity.

From a comparison of the length, breadth, and arch of the vertex of the skulls of the chimpanzee and of two specimens of hylobates, with their actual capacity as determined by measurement, and from the measurements of the length, breadth, and arch of the vertex of the fossil, M. Dubois arrives at the conclusion that the actual capacity of the fossil cranium had been about 1000 cubic centimetres, that is, about double the capacity of the cranium of the gorilla, and about two-thirds of the capacity of a well-formed European cranium. Dubois recognises that the Java calvaria approximates more to the human type than to that of the anthropoid apes; thus it is much more spacious, its vault is more highly arched, the supra-orbital arches are less projecting, the diameters generally are greater, and the downward and forward slope of the nuchal part of the occipital bone is more pronounced than in the Simiidae. Notwithstanding these human characters, he does not regard it as a human skull.

As the University Museum contains a number of examples of the crania of the larger anthropoid apes, as well as a large collection of human crania, illustrating the different races of men, I have thought that it would be useful to compare Dubois' description and measurements with these specimens.

As regards the glabello-occipital length, the Java calvaria is 54 mm. longer than the mean of two chimpanzee skulls, one of which is an old male; 54 mm. longer than a male orang, and 65 mm. longer than a female orang. It is more difficult to make a comparison with the skull of the gorilla, as in this animal the strongly projecting occipital crest gives a length out of all proportion to the proper glabello-occipital diameter. Thus in a remarkably fine male, the glabello-cristal length is 217 mm., whilst in an adult female with a slight crest it is 153 mm., and in a young specimen, where the occipital crest is only just indicated, the long diameter is 132 mm.

In its greatest breadth the Java fossil is 32 mm. broader than the greatest mean breadth of the two chimpanzees and the female orang. It is 21 mm. broader than the mean of five adult male gorillas, 30 mm. broader than the female, 35 mm. broader than the young specimen. The breadth of the apes' skulls was taken in the squamous region.

The frontal diameter behind the orbits was, in the Java fossil, 20 mm. greater than in the chimpanzees, and 28 mm. more than in the orangs. It was 19 mm. greater than the mean of five male gorillas, 24 mm. greater than in a female gorilla, and 22 mm. greater than in a young specimen. In the undamaged state of the Java fossil, as Dubois thinks, this diameter was probably 4 mm. greater than in the specimen as it now exists.

In the external dimensions of length and breadth, it is clear that the Java fossil is very much larger than the corresponding dimensions of the great anthropoid apes, except as regards the length of the largest skulls in the male gorilla, which are so materially elongated by the development of the occipital crests.

In the following Table the dimensions of the Java calvaria and of the anthropoid apes, which I have measured, is given :—

| | Age. | Sex. | Length. | Breadth. | Post Orb. Br. | Cub. Cap. |
|---|---|---|---|---|---|---|
| Java calvaria (*P. erectus ?*), | Ad. | ? | 185 | 130 | 90 | 1000 ? |
| Chimpanzee, . . | Ad. | ♂ | 132 | 98 | 70 | 350 |
| ,, (*calvus ?*), | Ad. | ♂ | 130 | 98 | 69 | 360 |
| Orang, . . . | Ad. | ♂ | 131 | 100 | 62 | 440 |
| ,, . . . | Ad. | ♀ | 120 | 98 | 62 | 360 |
| Gorilla, . . . | Ad. | ♂ | 190 | 113 | 72 | 480 |
| ,, . . . | Ad. | ♂ | 182 | 113 | 68 | 470 |
| ,, . . . | Ad. | ♂ | 205 | 105 | 73 | 520 |
| ,, . . . | Ad. | ♂ | 217 | 106 | 67 | 590 |
| ,, . . . | Ad. | ♂ | 176 | 107 | 74 | 410 |
| ,, . . . | Ad. | ♀ | 153 | 100 | 66 | 420 |
| ,, . . . | Young, | ? | 132 | 95 | 68 | 355 |

In the apes the length was inio-glabellar, but in the gorillas it included the crista occipitalis.

Dubois draws a sagittal line between the most projecting part of the glabella and the inferior curved line of the occiput, and traces the profile outline of the Java specimen. He states that the highest point of the cranial vault is 62 mm. higher than the sagittal horizontal line, a dimension which, to the long diameter of the skull, is as about 1 to 3. Although in its vault considerably lower than the European, it is, on the other hand, very appreciably higher than either in the chimpanzee or gibbons.

In comparing the length of the skull in the Java specimen with that of the anthropoid apes, it must be kept in mind that, although in the Java fossil the glabellar projection is stronger than in human crania generally, yet that, neither absolutely nor relatively, is it so prominent as in the skulls of the chimpanzee and gorilla.

In my memoir on human crania, in the Report of H.M.S. *Challenger*,* I described a method of taking the internal capacity of the skull, which seemed to me to give more precise results than those of Broca and other craniologists. I have employed this method in the determination of the capacity of the crania of the anthropoid apes, specified in the Table, and in taking the measurements I have on this, as on so many other occasions, been indebted to my Museum Assistant, Mr James Simpson. The adult male gorillas ranged from 410 to 590 c.c., giving a mean of 494 c.c. The Java skull possessed, therefore, according to Dubois' estimate, twice the capacity of the mean of the five male gorillas, and more than twice that of the female gorilla. It was two and a half times as capacious as the mean of the two orangs, and approached to three times the capacity of the skull of the chimpanzee.

In comparing the Java specimen with human crania, M. Dubois almost entirely limits himself to a comparison with the European skull. It is obvious, however, that to obtain a proper conception of its affinities, the comparison should not be restricted to highly developed European races, but rather it should be looked at side by side with a race now dwelling under savage conditions. There is no doubt that, as compared with a dominant European race, the cranial capacity of the Java specimen, if the accuracy of Dubois' estimate be accepted, is much below that of such a people, for example, as the modern Scot. Thus the capacity of the skulls of 50 Scotsmen in

* Zoology, Challenger Expedition, part xxix. p. 9, 1884.

the University Museum, taken according to the method to which I have already referred, gave a mean of 1492·8 c.c., and ranged from 1770 to 1240 c.c.; that of 23 Scotswomen had a mean of 1325 c.c., and ranged from 1625 to 1100 c.c. The mean of the Scotsmen closely approximates to Welcker's measurements of Europeans generally, quoted by Dubois, and places the fossil, in its capacity, as much below the European mean as it is above the mean capacity of the male gorilla.

If we now take the aboriginal Australians as an example of the modern savage, we find them to be a low-typed, purely dolichocephalic race presenting many features of correspondence with the Java specimen. The glabella and supra-orbital ridges are, in a large majority of Australian skulls, massive and projecting. A keel is not unfrequently found in the line of the obliterated frontal suture, and the vault of the cranium is, in many specimens, feebly arched. As regards the length of the skull, the mean glabello-occipital length of 25 Australian men was 190 mm., that of 13 women was 177 mm.—giving as the mean of the two sexes 183·5 mm., which almost exactly corresponds with the Java specimen. The greatest breadth of the Australian men was, on the average, 131 mm., and of the women 127 mm., so that the Java fossil practically corresponded in breadth to the men, and was slightly broader than in the women. The post-orbital frontal breadth was, on the average, 97·6 mm. in the men and 92 mm. in the women, which was slightly more than the breadth in the corresponding region in the fossil.

As regards internal capacity, it is very rare for an Australian skull to measure 1500 c.c., though I have measured a man from Queensland who reached 1514 c.c., one from the De Grey river 1450 c.c., and one from South Australia 1400 c.c. The average of 24 Australian men was, however, only 1286 c.c., and of 12 women 1106 c.c. In the men, no specimen was below 1000 c.c., but one was only 1044 c.c. In the women five specimens were below 1100 c.c., and three of these measured 930, 946, and 998 c.c. respectively. Granting, therefore, the accuracy of M. Dubois' estimate of 1000 c.c. for the fossil, and if it be as he supposes of the female sex, three Australian women were below it in capacity, and a considerable number were only a little more capacious.

In the skulls of other savage races in the University Museum, namely, Andaman Islanders, Admiralty Islanders, Bush people, Veddahs and hill tribes of India, I find 17 specimens ranging from 1000 to 1092 c.c. Two of these were probably males and the rest females. It follows, therefore, that a human cranium, smaller in its capacity than 1100 c.c., is yet sufficiently large for the lodgment of a brain, competent to discharge the duties demanded by the life of a savage.

*Upper Molar Tooth.*

The isolated tooth was found 1 metre distant from the calvaria. The crown is described as forming an unequal triangle, with one lateral and two median rounded angles; the base was turned forwards and a little concave; the transverse diameter of the corona at the base was 15·3 mm., and the greatest sagittal diameter on the inner side was 11·3 mm. In the direction from before backwards it was very short. On the one side, the two anterior cusps were tolerably well developed, but on the other side the posterior median cusp was much reduced, and the postero-lateral scarcely developed. In consequence of this, the connecting band between the anterior median and the postero-lateral cusp did not exist, and the hollows of the grinding surface were quite irregular. This surface was only slightly worn in places. The tooth had two strongly diverging fangs, which projected somewhat obliquely backwards, the obliquity being due, M. Dubois thinks, to the fact that there had not been much space for the tooth in the sagittal diameter of the jaw. The median root measured from the neck 13 mm.; it was transversely compressed; the lateral root was 15 mm. long; on the inner side it was broadly and deeply forked, owing to the fusion of an anterior shorter, and a posterior longer fang, both of which were compressed from before backwards. The form of the tooth indicated that, notwithstanding its great breadth, it had undergone a strong retrogression in the sagittal direction, which pointed to a corresponding retrogression in the entire dentary arcade. Dubois states that the tooth is larger than the corresponding molar in Man, and the grinding surface more rugose. On the other hand, it is not so strongly developed as in the gorilla and orang, nor so rugose.

In commenting on this description one is, in the first instance, disposed to raise the question whether the tooth belonged to the skull, the calvaria of which was found in its neighbourhood. From the fact that the grinding surface was only slightly worn, one would not be prepared to associate it with a skull, where all the sutures of the vault were so obliterated, as in the Java calvaria. As regards the size of its crown, I have compared it with the teeth of the anthropoid apes in the University Museum. It is somewhat larger than the third upper molar in the skulls both of the chimpanzee and orang. In the adult male orang the crown of the upper wisdom was 11 mm. in sagittal, and 13 mm. in transverse diameter; in the female the corresponding diameters were 9 mm. and 12 mm. It is almost equal in size to the corresponding tooth in one of the male gorillas, but it is distinctly smaller than in the three other males. Compared with the female gorilla, its diameter in one direction is 1·3 mm. greater, and in the other 1·7 mm. less. It was distinctly larger than the upper wisdom tooth in Europeans. Compared with the corresponding tooth in a number of Australian skulls, it was also greater in the dimensions of its crown; but in a male skull, from the Riverina district of N. S. Wales, the transverse diameter of the crown of the corresponding tooth was as high as 14 mm., and the sagittal diameter, on the inner side, was 9 mm., and on the outer 10 mm. It is, I think, by no means clear that this tooth is from a human jaw, and is not rather that of an anthropoid ape. That it belonged to a gorilla or chimpanzee is out of the question, as these apes are African and not Asiatic in their habitat. The question arises if it may not have been that of a large orang, in which case the area occupied by this ape would have been more extensive than Borneo and Sumatra, its present habitat, and would have included Java. The general configuration of the crown is indeed not unlike that of the wisdom tooth of an adult orang; though, without having the tooth before one for examination and comparison, one does not wish to express too positive an opinion.

*Left Femur.*

This isolated bone was found nearly 50 feet higher up the river bank than the calvaria. It was only slightly injured, at the head, great trochanter and the lower articular ends. It had, however, a large irregular pointed exostosis, growing from the inner and back part of the shaft, below the small trochanter. It was an adult bone, and its surface was not smoothed down by friction against extraneous objects. Its length from the highest point of the head to a line connecting the lowest points of the two condyles was 455 mm. In his description of the bone, M. Dubois recognises many features of correspondence with the human femur; in the shape of the head, that of the trochanters and of the anterior inter-trochanteric line; in the development of the ridge for the insertion of the gluteus maximus; in the angle formed by the neck with the shaft, in the compression of the neck, in the form of the lower articular end, and of the inter-condyloid fossa, and in the presence of a linea aspera, it repeats the human characters, so much so, indeed, that Dubois has no hesitation in concluding that the femur could be extended, both on the trunk and leg, as to admit of the erect attitude.

On the other hand, he states that it differs from the human thigh bone, in the absence of an *angulus medialis* or line separating the anterior convex surface from the inner surface; in the inner surface being convex and not concave; in the popliteal surface having less definite lateral boundaries, and being somewhat convex and not flat, whilst the inter-trochanteric crest is less elevated, narrower and turned inwards, so that this line is not straight, but concave. These supposed points of difference are, he considers, of sufficient moment to distinguish it from the human bone, and to approximate it to the femur of anthropoid apes. In arriving at this conclusion, M. Dubois has not had before him, for purposes of comparison, a sufficient number of human femora, and has not realised the variations which occur in the bone in those areas where he conceives that the Java specimen differs from the femur in Man. From the examination of a large number of femora, both European and exotic, I am able to state that the characters which Dubois considers not to be human are occasional varieties in the femur of Man, so that they lose all significance as marks of differentiation from the human femur.

There can be no doubt that the Java femur is a human bone, but whether it is the thigh bone of the skeleton to which the calvaria belonged is, I think, extremely doubtful. The distance at which it was found from the skull-cap and the fact that it was lying in an alluvium brought down in the course of a tropical river, show that the remains were only loosely associated with each other, and had not of necessity any organic connection. The sharpness of its contours and of the pointed exostosis are characters which it is difficult to reconcile with the condition presented by the calvaria. On the supposition that it was of the same age as the calvaria, as to which, however, there is, I think, some doubt, it showed few signs of rubbing or injury as compared with what the skull itself has suffered. Consequently, I am not disposed to think that the characters of the femur are of any moment in our interpretation of the skull-cap, which must be weighed on its own merits.

In the projection of the supra-orbital ridges and glabella, and in the shape of the occipital region, the Java calvaria bears such a resemblance to the Neanderthal skull that, the latter being regarded as human, one sees no reason why, in these respects, the Java fossil should not likewise be human. In both, also, the cranial vault has a low arch, though M. Dubois considers that, in this respect and in the internal capacity, the fossil is below the skull from the Neander Valley. As regards the capacity, the injured state of the specimen only admits of an approximate estimate, but, on the basis that it was about 1000 c.c., sufficient evidence has been adduced in this communication to show that, in the dolichocephalic aborigines of Australia, the crania in a number of instances were only slightly above that figure, and in some even below it, whilst in other savage races, an equally low capacity is occasionally found. In my judgment, therefore, there is nothing in this character to lead one to say that the skull was not a human skull. If we accept the view that the Pleistocene deposit in Java, in which this specimen was found, is of the same geologic age as the European Pleistocene, there is nothing in the configuration of the skull-cap to place it in a different category from those remains of human Quaternary Man obtained in Europe, which have already been referred to as possessing similar characters.

From the above criticism it will be seen that I am unable to accept M. Dubois' opinion that we have in these remains evidence of a new genus and species intermediate between man and apes. The existence of such a transitional form is still a matter of speculation, and has not been placed on the basis of ascertained fact.

## On Drops. By J. B. Hannay.

(Read May 6, 1895.)

The formation of drops, their variation with density and chemical composition of the liquid forming them, and their variation with temperature and frequency, have been the subject of investigation by several chemists and physicists, yet investigators are by no means at one as to the definition of a drop or the cause of its parting from the flowing liquid.

The first investigation by which it was sought to find a definition of a standard drop, and to discover the laws regulating its formation, was that carried out by Guthrie,* who was, however, driven to the conclusion that no perfect drop exists, but that every drop is more or less imperfect. It is well known that the quicker the rate of dropping the larger is the size of the delivered drop; and Guthrie explained this by supposing that when a drop parts from a solid support, a portion of the root or stem of the drop is torn back by the attraction of the solid from which the drop falls. Guthrie allowed his drops to fall from a rounded surface, and he supposed that when the flowing of the liquid to the solid was quick the attraction of the solid for the liquid was more fully satisfied, and hence a smaller portion of the root of the drop was torn back to satisfy the attraction of the solid.

According to Guthrie, a full normal drop could not exist, all drops being more or less imperfect.

This conclusion may be tested by making a liquid drop from itself, by which the question of the attraction of a solid is entirely eliminated, and this may be done in two ways. The first is to ascertain whether or not there is a variation in the size of the drops into which a smooth running stream divides as it falls, when the rate is varied. This is a case of a liquid entirely dissociated from any solid. The second method is to allow a liquid to drop from a column of its own substance retained in position by retaining walls to which the liquid does not adhere. This second condition may be easily attained by using mercury as the liquid, and dropping it from

* "On Drops," *Proc. Roy. Soc.*, vol. xiii.

a dry clean glass tube of the greatest width which is compatible with preventing the air entering and the mercury running out.

An attempt was made to ascertain the size of the drops into which a stream of varying velocity divides, by counting them by means of an apparatus which I described in 1878,* by which the falling drops actuated a pen which recorded each drop on a revolving cylinder. While it was easily proven that the same law holds good for drops formed in this manner as for those dropping from a solid, the difficulties of manipulation rendered this method of little value for accurate work.

The second method was carefully investigated, and the results are fully detailed in the paper referred to, in which it was clearly shown that a liquid dropping from itself (sustained in column form by walls which it did not wet) gave the same variation of size with rate as when dropped from a solid.

This method absolutely precludes the idea of a portion of the drop being torn back by adhesion to the solid, as there is no solid to which the mercury could adhere.

The conclusion arrived at was that the neck of a drop forms a tube through which liquid is flowing into the drop, and after a drop begins to fall it receives an accession of liquid during the time taken to complete the rupture of the neck, this accession varying directly as the flow. Besides this, it seemed probable that owing to the "stump" of the drop following after the falling drop the life-time of the neck might be lengthened.

This led to a conclusion diametrically the opposite to that of Guthrie—viz., that a true drop is one of which the rate is infinitely slow, and all other drops are greater than a true drop, and not less, as Guthrie concluded.

The weight of a true or normal drop is therefore easily found by determining the difference of weight with rate, and reducing the rate to zero, which then gives the weight of a drop at the moment it begins to part.

If it be true that the increase in the weight of a drop with rate is due to the influx of liquid through the neck while rupture is taking place, it is clear that the size of drops will not be accurately

* *Trans. Roy. Soc. Edin.*, vol. xxiii. 697.

related to any of the chemical or physical properties, but must also depend partly on the mechanics of flow.

That the size of drops of solutions of salts in water has no connection with the viscosity or internal friction, is easily shown by comparing the numbers obtained by experiments on friction of that kind detailed in a paper published in 1879,* with the variation in the weight of drops of such solutions. Thus some solutions greatly increase the rate of flow, while others retard it; but no connection is directly traceable between such rates and the size of the drops, as shown in the following table:—

| Salt in Solution. | Strength. | Time. | Difference. | Weight of Drop. |
|---|---|---|---|---|
| ... | Water alone | 370″ | 0″ | 0·1040 grm. |
| KI | 4 times normal | 243″ | −127″ | 0·1094 ,, |
| ,, | 2 times normal | 297″ | − 73″ | 0·1076 ,, expl. |
| ,, | Normal | 312″ | − 58″ | 0·1063 ,, |
| $KNO_3$ | ,, | 340″ | − 30″ | 0·1080 ,, sp. gr. |
| $\frac{1}{2}K_2SO_4$ | ,, | 394″ | + 24″ | 0·1071 ,, |
| $\frac{1}{2}MgSO_4$ | ,, | 490″ | +120″ | 0·1070 ,, |

Here we see that KI and $KNO_3$ increase the rate of flow or diminish the internal friction, while $K_2SO_4$ and $MgSO_4$ decrease the rate of flow; yet the KI solution of double the normal strength and $MgSO_4$ of normal strength form almost identical drops.

It therefore became important to minutely examine the conditions under which a drop falls, especially the time taken for rupture, and to determine accurately what occurs during the rupture of the neck. It was found that to obtain constant results it was essential to have the most absolute freedom from impurity; and the tube from which the drops fell was heated almost to its softening point after each change of solution, so that any grease or impurity which might have contaminated it was entirely destroyed. The method of regulating the rate of dropping was to cause the solution to drop at the lowest rate required by interposing a capillary tube in the flow, and then increasing the rate to any extent required by air pressure. In this way any required rate could be at once obtained.

In order to study the conditions under which a drop parts from its root or stem, water was allowed to drop through oil of different

* "On the Microrheometer," *Trans. Roy. Soc.*

densities and viscosities, and oils were allowed to drop up through water, so that by the slow parting of such drops the method of parting could be investigated.

As the experiments of dropping water in pure olive oil gave very constant results, I will use the numbers obtained in this way in illustration of my conclusions.

The conditions were studied by dropping distilled water from a tube 5·9 mm. internal and 7·6 mm. external diameter, at a temperature of 20°, in saturated air and in pure olive oil. It was clearly seen when dropping in oil that the rupture of the drop from the end of its supporting column was an operation taking some time, and from the moment the neck began to narrow till it parted was about 2·8 seconds. When the rate was quicker there was very little difference in the time taken to complete the rupture, and at the highest rate at which observations could be made it was not over 3 seconds. Then it was also seen that the root of the drop left behind was always the same size no matter what the rate. The stump was measured by viewing with a cathetometer, and the following numbers obtained when dropping oil in air :—

| Growth Time of Drop. | Length of Stump. Mean of Ten Observations. |
|---|---|
| 12″ | 78° on scale. |
| 7·5″ | 77° ,, |
| 4·3″ | 78° ,, |
| 1·9″ | 79° ,, |
| 0·6″ | 80° ,, |
| 0·3″ | 78° ,, |

From this we see that all the liquid which flows down during the rupture of the neck passes into the drop, and that the neck parts at the highest point.

The cause of the rupture of the neck was then studied, and it was found that it was due to two causes—namely, surface tension and gravity. This can be proved in several ways. When water is delivered in a solid stream into oil from a tube of the same diameter as that from which the drops were produced, and if, during delivery, the tube be raised at a suitable speed, then the water can be delivered into the oil as a straight rod (as in fig. 1*a*). But at intervals along this rod there rapidly form constrictions, generally somewhat irregular; but when the stream has been delivered very steadily, often quite regular (as at fig. 1*b*), which quickly close up just as at the root of a drop, and divide the stream into drops (as at *c*). In this case, as the rod as a whole is gently falling, and all parts are equally acted upon by gravity, there is no force pulling the rod asunder, yet it divides into drops similar to those formed when *dropping* from a similar tube. The size of a normal drop in oil is ·4096 c.c., and when dropping at the rate of 10″ to a drop the volume is increased to ·5611 c.c.; while the drops formed from a cylinder of water as above, have a volume of ·5470, which shows that the determining factor in the formation and parting of a drop is surface tension.

The part played by gravity in determining the weight at which a drop will fall consists in its downward pull tending to deform a drop which without gravity would be a sphere, as can be seen by dropping a non-miscible pair of liquids of equal density one in the other, when the drop grows indefinitely into a large sphere. When gravity pulls the drop out of its spherical form and forms a cylindrical neck, the contractile force of the liquid surface will not permit of this form, but at once starts a constriction which shears through the neck and detaches a drop.

Some experiments which throw light on the function of the neck of the drop may be detailed here. If, in forming the rod-like drop of water in oil (as at fig. 1*a*), we stop the upward motion of the delivery tube while the flow continues, we obtain a formation like fig. 2*a*. The resistance of the oil to the fall of the water causes an accumulation at the lower end of the elongated neck in the form of a large drop. The long neck may be maintained intact so long as a sufficient flow is kept up, but when it becomes long it wriggles like something endowed with life, and the surface tension or contractility causes constrictions to appear and shift from place to place without causing any rupture (as at fig. 2*b*).

When the stream is diminished the contractile force overcomes the power of the stream to keep the tube open, and the tube is severed (as at fig. 2*c*), a new bulb forming higher up; or if the flow be suddenly stopped (as at *d*), the entire stem separates into drops. The division is seldom very regular, as the neck is in active wriggling motion at the moment of rupture.

In order to study the conditions of parting, water was dropped in oil and in air, and mercury was also dropped in air, so as to obtain three different conditions of parting.

The following are the numbers for water in oil:—

| Growth Time of Drop. | Vol. of Drop. | Quantity passing through Neck after Rupture had begun. | Residue. |
|---|---|---|---|
| 120″ | ·4096 | ·0091 | ·4005 |
| 27″ | ·4607 | ·0570 | ·4037 |
| 11·3″ | ·5611 | ·1479 | ·4132 |

The quantity passing through the neck was calculated by multiplying the rate of flow by the time of rupture of the neck,—3 seconds. By subtracting this quantity from the observed drop we ought to get a normal drop, so that the figures in the column headed "residue" ought to be exactly the same, and we see that is not the case. After some study the cause was found in the viscosity of the oil, which resists the downward movement of the growing drop; so that on measuring the extreme width of the drops with a cathetometer at different rates, it was found that the higher the rate the larger was the drop, as shown by its greater diameter.

In the case of water dropping in air, this effect had quite disappeared, while in the case of mercury in air an effect of an opposite character was manifested.

In the case of water dropping in oil, the time taken to cut off the neck after the drop was ripe could be measured with ease, and was found to be independent of rate; but in the case of mercury dropping in air direct observation could not be made, so the time of rupture of the neck was calculated by finding how long the flow must have continued through the closing neck in order to account for the increase in weight.

| Rate of Flow. | | Calculated Time Occupied by Rupture. | | Addition to Drop through. | | Normal Drop. | | Observed Drop at given Rate. |
|---|---|---|---|---|---|---|---|---|
| 6·431 | × | ·036″ | = | ·2315 | + | 4119 | = | ·6434 |
| 4·594 | × | ·042″ | = | ·1884 | + | 4119 | = | ·6003 |
| 2·450 | × | ·0515″ | = | ·1262 | + | 4119 | = | ·5381 |
| 1·252 | × | ·0707″ | = | ·0885 | + | 4119 | = | ·5004 |
| 0·5788 | × | ·087″ | = | ·0504 | + | 4119 | = | ·4623 |
| 0·4598 | × | ·104″ | = | ·0478 | + | 4119 | = | ·4597 |

From these numbers we see that the quicker the rate the shorter must have been the duration of the neck after rupture had commenced. This apparently anomalous decrease of the time arises from the same cause as the opposite effect in the case of water in oil—*i.e.* it is connected with the motion of the forming drop. We saw that the viscosity of the oil buoyed up the water drop, and so caused undue increase in the size of the drop, independent of its increase through the neck. In the case of mercury in air, the time of the rupture of the neck actually varies, the shape and size of the ripe drop being always the same. The reason of the varying life-time of the neck is found in the downward velocity of the parting drop. When the rate is very slow the mercury composing the drop starts to fall from rest, and the rate of the closing of the neck in this case is entirely controlled by the contractility or surface tension; but as the rate increases, the mercury is already in downward motion at the moment of its beginning to part, so that its velocity of descent is greater the quicker the rate, and it actually tears the drop away from its root, and closes the neck more quickly than would result from the action of the contractile power of the liquid; and the great density of the mercury emphasises this action.

The effect of gravity on the formation of drops can be very prettily illustrated when dropping water in oil by introducing (by means of a small pipette with a capillary delivery point) some dense solution into the interior of the drop, when a small drop will form and detach itself (as shown in fig. 3, *a*, *b* and *c*).

The conclusions at which I have arrived by an examination of the closing neck are similar to those held by Tate (*Phil. Mag.* 1864), who concludes that "the weight of the drop is sensibly proportional to the diameter of the tube from which it falls, hence the force which holds the drop is a surface one, *and not one of general cohesion.*"

This is clearly shown by comparing the weights at which the neck of a water-drop is ruptured when dropping in air or in oil from the same tube. Taking a wide tube, the weight in air is 0·178 grm. In oil the neck is ruptured by a weight of only 0·052 grm., or about one-third. The drop, however, has a volume of 0·585 c.c., or fully three times that of water in air. In this case, the material of the neck is identical, and the interior water is in no way affected; while the neck of the water-in-oil drop is wider than that of the water-in-air drop, yet the force required to produce the conditions for rupture is only one-third. The only change in the condition is that of the outer skin of the drop. Had the liquid any

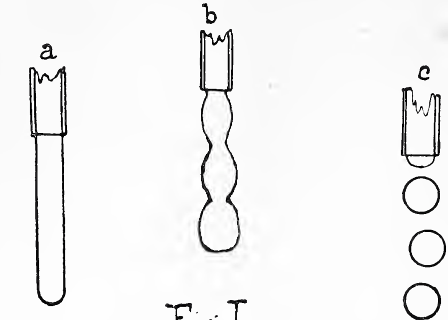



Fig I.

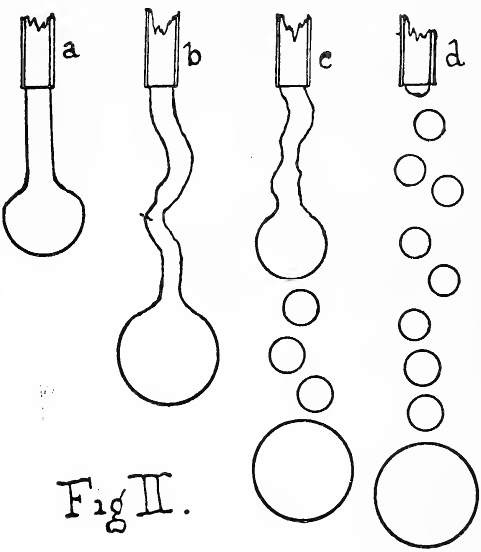



Fig II.

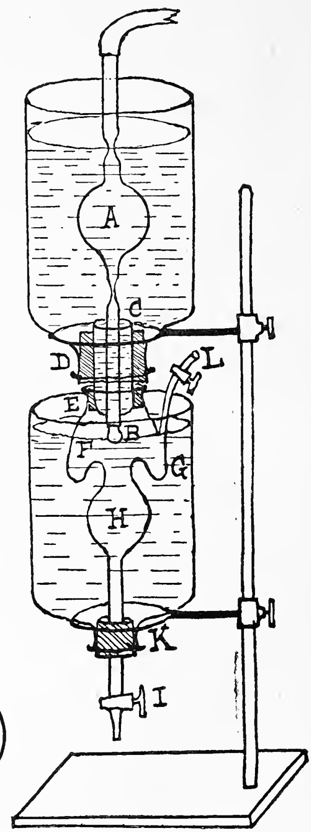



Fig IV.

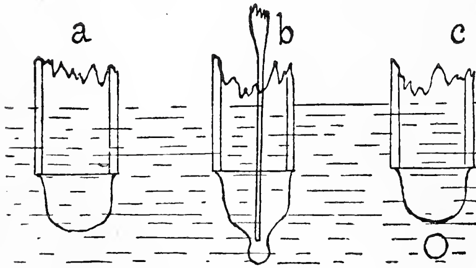



Fig III.

property of cohesion like solids, the pull required to rupture the neck would depend on the diameter of the neck, and would be independent of the medium in which the drop was formed.

A similar conclusion was arrived at by Duclaux (*ann. Ch. Phys.* [4] xxi.), who points out that it is only an extremely thin envelope of the drop which influences its size and shape. Summing up, he says:—"Il n'est pas dificile de montrer que, non seulement l'influence de cette couche est considerable mais qu'elle est predominante, et que la cohésion du liquid ou en général toute force dependante des surfaces en contact n'a pas d'action sensible."

Duclaux shows that the weight of a drop is profoundly modified by the condition of the outer skin; and by dropping water in a vessel whose walls were wet with alcohol, he showed that the diminution of the size of the drop was so great that it could only be accounted for by supposing that the alcohol formed a thin layer on the drop, which then acted as though its entire bulk were of the same composition as the outer skin. It appeared to me that this question might be more accurately examined by employing a liquid insoluble in water to form the film. An apparatus was fitted up as at fig. 4, where A is the measuring bulb and B the tube from which the drops fall. This tube was sealed into a wider tube C, which served to keep the dropping tube surrounded by water although passing through the rubber stoppers D and E.

A lower vessel F, formed with a side well G, in which lay the liquid through whose vapour the drops were to fall, was fitted by a rubber stopper to the dropping tube B, having a lower bulb H, fitted with a stopcock I, sealed on after the stopper K was passed over the tube, so that the dropped liquid might be quickly removed for examination.

A tube L with a stopcock gave free access to the air, so that the pressure throughout was that of the atmosphere, and it also served to introduce the liquid in whose vapour the drops were to be found.

Two liquids were used,—Absolute Alcohol and Benzine.

The temperature chosen was 37°, so that there might be a considerable quantity of the vapour present, and as the water was dropped at 20° there was a tendency for the vapour to quickly condense on the cooler drop.

The weight of the normal water drop was 0·1081.

| | | | | |
|---|---|---|---|---|
| ,, | ,, | alcohol | ,, | 0·0307. |
| ,, | ,, | benzine | ,, | 0·0449. |

The water was then dropped in the vapours at different rates, and the weights corrected so as to reduce them to what they would have been at zero rate so that they might be comparable :—

| Growth Time. | Water in Vapours at 37°. | |
|---|---|---|
| | Alcohol. | Benzine. |
| 1″ | 0·0983 | 0·0940 |
| 17·5″ | 0·0947 | 0·0816 |
| 50″ | 0·0928 | 0·0613 |

We see clearly the influence of allowing time for the condensation of the vapour in the decreasing drop with increasing growth-time ; but in the case of alcohol the effect is not so marked, as the alcohol mixes with the water, and hence the surface is continually renewed, whereas the benzine, being practically insoluble, accumulates entirely on the surface.

The liquids were raised to a temperature within a few degrees of their boiling points, and the water dropped more quickly, with the following results :—

| Growth Time 0·5″. | |
|---|---|
| Water in Alcohol Vapour. | Water in Benzine Vapour. |
| 0·0326 | 0·0534 |

It will be seen that notwithstanding the solubility of the alcohol in the water, its action when there is a sufficient supply is more potent than the benzine.

This is owing to its solubility. The capillary height of two non-miscible liquids in the same tube depends not only on the specific gravities of the two liquids and the capillarity of the upper liquid, but also on the action of the meniscus separating the two liquids; whereas, when the liquids are freely miscible, the height depends only on the mean density and the capillary action of the upper surface.

Hence with the benzine experiment the drop (had the capillarity of benzine controlled it) ought to have weighed ·0449, whereas, owing to the limiting surface between the benzine and water, it weighed ·0534, an increase of 18·9 per cent. ; whereas with alcohol the weight was ·0326 instead of ·0307, an increase of 6·1 per cent., no doubt due to the surface layer containing a trace of water. The drops were reduced to such a size as would represent alcohol of 94 per cent.; whereas, on analysis, they contained only 5·62 per cent. of alcohol.

Tranbe (*annalen* 265, 27–55), by an examination of certain organic alcohols and acids, concludes that the volume of the drops is proportional to the rise in height in the capillary tube, and the principal object in the present investigation was to examine this relationship when saline solutions are employed. The drops were calculated to zero rate and the experiments conducted at 20°, water being taken as 100 in each case :—

| Salts. | Solution. | Drop (Water 100). | Capillary Constant (Water 100). |
|---|---|---|---|
| KI | Normal | 101·3 | 101·3 |
| $\frac{1}{2}MgSO_4$ | ,, | 103·4 | 103·2 |
| $\frac{1}{2}Na_2SO_4$ | ,, | 100·4 | 100·3 |
| $KNO_3$ | ,, | 103·0 | 102·8 |
| $NH_4CL$ | 4 times normal | 108·1 | 108·3 |

From these numbers it is clear that the size of the drop is in close agreement with the capillary constant.

In conclusion, then, the weight of a normal or infinitely slow drop is controlled by its surface tension or contractility; while, when dropping in practice, it is modified by the rate of flow, by its gravity, the viscosity of the medium in which it drops, and by its rate of fall, all of which affect the life-time of the closing neck.

## On the Rendering of Animals Immune against the Venom of the Cobra and other Serpents; and on the Antidotal Properties of the Blood-Serum of the Immunized Animals. By Professor Thomas R. Fraser, M.D., F.R.S. (With a Diagram.)

(Read June 3, 1895.)

(*Abstract.*)

One of the most striking and interesting of the many traditions and current beliefs regarding venomous serpents is that a power may be acquired of freely handling them without injury, and even of successfully resisting the poisonous effects of their bites.

The Psylli of Africa, the Marsi of Italy, the Gouni of India, and other ancient tribes and sects, were stated to have been immune against serpents' bites, and to have been able to exercise a remarkable influence over even the most venomous of these animals; and these attributes have been explained on the supposition that serpents' blood was present in the veins of the members of these tribes and sects.

In more modern times, and, indeed, at the present day, the same belief is stated in the writings of travellers; and it has been expressed by poets and novelists, and among the latter, with a half-admitted conviction of its reality, by Wendell Holmes, in his *Romance of Destiny.*

In "a new and accurate Description of the Coast of Guinea," published in 1705, by William Bosman, an account is given of the great "reverence and respect" of the negroes for snakes worshipped by them as gods; in connection with which the following statements are made :—"But what is best of all, is, that these idolatrous snakes don't do the least mischief in the world to mankind; for, if by chance in the dark one treads upon them, and they bite or sting him, it is not more prejudicial than the sting of millepedes. Wherefore, the natives would fain persuade us that it is good to be bitten or stung by these snakes, upon the plea that one is thereby secured and protected from the sting of any poisonous snake. But here," he proceeds to remark, "I am somewhat dubious, and should be loth to venture on the credit of their assertions, because I have observed that the gods themselves are not proof against these venomous serpents, much less can they protect us against their bite."

Drummond Hay, in his work on Western Barbary, gives a description of the performances of four members of a sect of snake-charmers, called the Eisowy (Aissaivi), who freely handled, and allowed themselves to be bitten by serpents proved to be venomous by a rapidly fatal experiment performed on a fowl. At the termination of the exhibition, the Eisowy, apparently as a usual performance, "commenced eating or rather chewing" a poisonous snake, "which, writhing with pain (to quote Mr Hay's words) bit him in the neck and hands until it was actually destroyed by the Eisowy's teeth." He states that, on another occasion, at Tangier, a young Moor, who was witnessing the performances of a snake-charmer, ridiculed his exhibition as a delusion, and having been dared by the Eisowy to touch one of the serpents the lad did so, was bitten by one of them, and shortly afterwards expired. In connection with my subject, a special interest is attached to the account given by Mr Drummond Hay, and repeated in its main features by Quedenfeldt in the *Zeitschrift für Ethnologie* of 1886, of the origin of this Eisowy (Aissaivi) sect, and of the immunity which they claim. The founder, Seedna Eiser, was being followed through the desert of Soos by a great multitude, who, becoming hungry, clamoured for bread. On this, Seedna Eiser became enraged, and turning upon them he uttered a common Arabic curse, "kool sim," which means "eat poison." So great was their faith in the teaching of the saint, that they acted upon the literal interpretation of his words, and thereafter ate venomous snakes and reptiles; and from that time they themselves and their descendants have been immune against serpents' bites.

In the writings of many other travellers similar evidences may be found of a belief in the possession of a power successfully to resist the poisonous effects of serpents' bites. The same belief presents itself in the conviction, prevailing in several parts of the world, that a non-fatal bite by a poisonous serpent, provided marked symptoms have followed, confers protection against subsequent bites; and in the tales of the performances by the snake-charmers of the present time. These performances have been graphically described, among others, by Hooker and Ball in their *Journal of a Tour in Marocco and the Great Atlas*, but only to be dismissed as impostures, rendered possible by the previous extraction of the poison-fangs, or by some other disabling operation. Although, possibly, the performances are at times, or even frequently, impostures, it almost appears as if this conclusion were arrived at more because of their improbability and their apparent defiance of knowledge regarding the effects of serpents' venom, than because of satisfactory or sufficient proof having been obtained of the conclusion. Some of the facts which I shall bring before the Society will, on the other hand, show that this conclusion can no longer be justified on the ground that the asserted facts imply impossibilities.

It may be instructive to associate with this belief in the possession, under certain conditions, by human beings of a power successfully to resist the poisonous effects of serpents' venom, and with the evidences in its support, the further belief that venomous serpents are themselves protected against the effects of bites inflicted upon them by individuals both of their own and of other species. On mere anatomical grounds it is difficult to understand how serpents could escape the absorption of their own venom through mucous surfaces, even admitting that absorption of venom does not occur in normal conditions of these surfaces. Venom must, however, be so frequently introduced into their bodies, in situations where absorption could not fail to occur, by the bites inflicted upon them by other serpents, that the conclusion seems inevitable that they possess some protective quality, without which, probably, no venomous serpents would now be in existence. Not only have many general observations been made in support of this belief, but it has been proved to be correct by direct experiments, such as those made by Fontana of Tuscany more than a century ago, and by Guyon, Laçerda, Waddell, Kaufmann, and Sir Joseph Fayrer.

This, and other evidence, pointing to the existence of protection against venom, not only in serpents themselves, but also, in certain exceptional circumstances, in human beings, several years ago originated a wish to further investigate the matter. It was obviously suggested that if protection occurs, it must be caused by some direct result of the absorption of venom; and, therefore, that its existence could be proved or disproved by experiment. In the former event, the first steps would already have been taken to obtain, by further experiments, results likely to be of value in the treatment of poisoning by serpents' venom; and, indeed, likely to be of importance in even the wider field of general therapeutics.

With these objects, endeavours were made to collect a sufficient quantity of venom; but the collection has proceeded but slowly, and only after several years has a supply gradually been accumulated sufficiently large to render it probable that definite results would be obtained before the supply of venom had become exhausted in the experiments.

I received my first supply of cobra venom in 1879, from Surgeon-Colonel Moir, lately of Meerut, and afterwards—also in small quantities—from the late Dr Shortt of Madras, and from Sir Joseph Fayrer, the Thakore of Gondal, and Dr Phillips. Larger quantities were subsequently obtained from Surgeon-Captain French, and through the kind efforts of Sir William Mackinnon, Director-General of the Army Medical Department, from each of the Presidencies of India. Early in this year, an additional supply was received from Surgeon-Colonel Cunningham of Calcutta, and this gentleman has quite recently sent a further large quantity of several grammes of dry venom.

But, besides these specimens of the venom of the cobra of India, I have also been fortunate in obtaining specimens of venoms from other parts of the world.

From America, Dr Weir Mitchell of Philadelphia—whose work on the chemistry and physiology of serpents' venom constitutes the great advance of the century on the venom of viperine serpents—has supplied me with the venom of three species of rattlesnakes—viz., *Crotalus horridus*, *C. adamanteus*, and *C. durrisus*, and also with a specimen of the venom of the Copper Head (*Trigonocephalus contortrix*).

From Australia, Dr Thomas Bancroft, of Brisbane, has at various times sent specimens of the venoms of the black snake (*Pseudechis porphyriacus*), the brown snake (*Diemenia superciliosa*), and of a large unidentified snake of the Diamantina district of South Australia (probably a new species of Diemenia).

From Africa, the kindness of Mr Wm. Smith, a distinguished naturalist of Cape Town, of Dr Brock of the Orange Free States, and of Dr John Murray and Mr Van Putten of Cape Colony, has placed at my disposal small quantities of the venom of the puff adder (*Vipera arietans*), the night adder (*Aspidelaps lubricus*), the yellow cobra (*Naja haie*), and the "Ring Hals Slang" or "Rinkas" (*Sepedon hæmachates*); and Dr John Anderson, formerly Professor of Natural History at Calcutta, has, only last week, forwarded to me living specimens of the *Vipera cerastes*, to be followed by living specimens of the cobra, which his present connection with the zoology of Egypt has given him peculiar facilities to obtain.

In the meantime, however, further evidence has been obtained in support of the reality of the probabilities to which I have referred. Sewall, using the venom of the rattlesnake, Kanthack that of the cobra, and Kaufmann and Phisalix and Bertrand that of the viper, obtained experimental evidence of the possibility of producing by "inoculation" a certain slight degree of resistance against the toxic effects of these venoms. The relationship of such observations to the recent discoveries in connection with the toxines of Tetanus, Diphtheria and other diseases, could not long remain unrecognised. Dr Bancroft and others have recently suggested "that the blood of animals rendered immune to snake venom might be found of service as a remedy in snake-bite." Within the last few months, Phisalix and Bertrand have obtained experimental indications of the antidotal power of the blood-serum of animals immunized, but only to a low degree, against the venom of vipers; while Calmette, working in the Pasteur Institute of Paris, after several unsuccessful endeavours had led him to express the opinion that immunity against snake venom could not be produced, afterwards succeeded in obtaining evidence of its production, and of the power of the blood-serum to counteract the effects of venom.

In the case of many of the venoms which I have had the good fortune to obtain, the quantity at my disposal was not sufficient for experimental examination on the plan that seemed desirable, and, besides, the examination of each of them would require several months of work. In this, the first portion of the investigation, therefore, the venoms that have been used are only four in number, those, namely, of the cobra of India (*Naja tripudians*), of the *Crotalus horridus* of America, of a large colubrine snake, probably a species of *Diemenia*, from South Australia, and of the *Sepedon hæmachates* of Africa. The venoms are therefore those of the most deadly of the poisonous serpents of Asia, America, Australia, and Africa, respectively; and further, they are representative of the chief differences that occur in the composition and action of venoms, for they are derived from members of the two great groups of the colubrine and viperine serpents.

My supply of cobra venom being much larger than that of any of the others, this venom was chiefly used in the experiments; and in all of those to be referred to to-night, the administration was effected by subcutaneous injection.

An essential preliminary to exact investigations with active substances must always be the determination of the activity of the substances. The only convenient method for doing this is to define the smallest dose capable of producing death for any given weight of animal—that is, the minimum-lethal dose. The venoms in their natural state are inconstant in activity, mainly because of variations in the quantity of the water which they contain. The cobra venom has, however, nearly always been received in the form of a dry solid; but when this was not so, it has been dried *in vacuo* over sulphuric acid.

Outside of India, there are few persons skilled in the hazardous task of taking venom directly from living serpents. Accordingly, with a few exceptions, the other venoms were not received in a pure form, but in the form of the dried venom glands. From these glands, however, the poisonous constituents may easily be extracted with water, and, on evaporating the solution over sulphuric acid, an active dry venom is obtained, containing, however, other substances besides those which are active. I am not in a position, therefore, to make any statement in regard to the relative activity of the different venoms. For the objects in view, what only is necessary is that the exact minimum-lethal dose should be known of each venom in the state in which it is used, whether it be pure or diluted with a certain small amount of inert matter.

Each of the four venoms was, however, found to be very active, but the cobra venom especially so,—a part of the difference between its activity and that of the other venoms being, no doubt, due to the above circumstances.

Experiments were made with cobra-venom on several animals—as the guinea-pig, rabbit, white rat, cat, and the innocuous grass snake of Italy (*Tropedonotus natrix*). Very considerable differences were found to occur in the minimum-lethal dose for these animals. For the guinea-pig, the minimum-lethal dose per kilogramme was ·00018 grm.; for the rabbit, ·000245 grm.; for the white rat, ·00025 grm.; for the cat, somewhat less than ·005 grm.; and for the grass snake, the relatively large dose of ·03 grm.* Cobra venom thus takes a position among the most active of known substances, rivalling in its lethal power the most potent of the vegetable active principles, such as aconitine, strophanthin, or acokantherin.

These facts having been ascertained, attempts were next made to render animals proof against lethal doses, by administering to them a succession of gradually increasing non-lethal doses. These were, for the first few doses, in some of the experiments, one-tenth of the minimum-lethal, in others one-fifth, in others one-half of the minimum-lethal, and in others almost as great as the minimum-lethal. At varying intervals, the doses were repeated, and by-and-by gradually increased, until the actual minimum-lethal had been attained. The subsequent doses, by gradual increments, exceeded the minimum-lethal, and after five or six times the minimum-lethal had been reached, it was found that the increments could be further increased, so that each became twice, four times, and latterly even five times the minimum-lethal, and still the animal suffered little, and, in many cases, no appreciable injury.

This brief statement, however, does not represent the experimental difficulties that were encountered. It describes the course of events in the altogether successful experiments. Non-success, however, was frequent, and many failures occurred before experience indicated the precautions and conditions that are necessary for success.

Serpents' venom exerts what may broadly be described as a duplex action. It produces unseen functional disturbances, and it

| | | | | |
|---|---|---|---|---|
| * Guinea-pig, | nearly $\frac{1}{5}$th millig. | | Kitten (6 weeks), | 2 millig. |
| Rabbit, | nearly $\frac{1}{4}$th millig. | | Cat, | 5 millig. |
| White Rat, | $\frac{1}{4}$th millig. | | Grass Snake, | 3 centig. |

also produces visible changes. The latter are of a highly irritative character, causing intense visceral congestions in the lungs, kidneys and other organs, and, when given by subcutaneous injection, on all the structures of the skin and subjacent parts. There are apparently also some definite changes produced in the blood, with regard to which several important facts have been discovered by Dr Martin of the University of Sydney. Irritative effects are obviously produced by cobra venom, even in non-lethal doses, and with greatly increased virulence by doses that exceed the minimum-lethal; but, in respect to this action, the other three venoms used are greatly more active than the venom of the cobra. Evidence was obtained to indicate that in the process of immunization, a diminution occurs in the intensity of these local actions; but this diminution does not proceed so rapidly as that in the unseen functional or other changes which are the more direct causes of death; and, further, the local irritative changes, after having been produced, are slower to disappear than the unseen functional disturbances. Until these facts had been appreciated, and, indeed, even with the adoption of precautions suggested by them, frequent failures occurred. The apparently contradictory results, accordingly, were obtained of the production, by gradually increasing doses, on the one hand, of a protection against quantities much above the minimum-lethal, so perfect that no apparent injury was caused; and, on the other hand, of an intolerance so decided that death was produced by the last of a succession of gradually increasing doses, no one of which was so large as the minimum-lethal. The latter unfortunate event was frequently displayed in guinea-pigs, and attempts to carry immunization in them to a high point were found to be extremely difficult.

Notwithstanding these difficulties, however, such gratifying results have been obtained as that rabbits could at last receive, by subcutaneous injection, so much as ten, twenty, thirty, and even the remarkable quantity of fifty times the minimum-lethal dose, without manifesting any obvious symptoms of poisoning. (See Diagram.)

Almost the only observable phenomena were a rise in the body temperature, which continued for a few hours after the injection, and which contrasts with the fall that occurs, after the administration of even non-lethal doses, in non-protected animals; and a loss of appetite, which usually, though not invariably occurred, and was probably the cause of a temporary slight fall in weight during the day or two days succeeding each injection. On the other hand, during the process of successful immunization, the animals increased in weight, they fed well, and appeared to acquire increased vigour and liveliness. This has been frequently exemplified in the smaller animals, such as rabbits; and also, very conspicuously, in an aged and previously sedate horse, which, in the process of immunization, has now received eleven times the estimated minimum-lethal dose.

It is marvellous to observe these evidences of the absence of injurious effects, and even of the production of benefit in an animal which, for instance, has received in one single dose a quantity of venom sufficient to kill, in less than six hours, fifty animals of the same weight, and, in the course of five or six months, a total quantity of venom sufficient to destroy the lives of 370 animals of the same species and weight. There are few facts in the whole range of biology more calculated to arrest the attention or produce astonishment in the mind of the observer!

With the cobra venom, I have also immunized cats, both by subcutaneous and by stomach administration; but the significance of the latter method of administration must be reserved for a future communication. As I have stated, a horse is also being immunized; and I have to express my obligations to Principal Williams and Professor W. Owen Williams, for granting me the accommodation of their establishment, and to Mr Davis, M.R.C.V.S., also of the New Veterinary College, for much valuable assistance.

Following the same plan of research with the three other venoms, it was found that the minimum-lethal dose per kilogramme for rabbits of the Diamantina venom is ·0015 grm.; of the venom of *Sepedon hæmachates*, ·0025 grm.; and of the venom of Crotalus ·004 grm.* The Crotalus venom is, in its purity, altogether comparable with the cobra venom; and the determinations, therefore, show that cobra venom is sixteen times more powerful than Crotalus or rattlesnake venom. This venom, as well as the two others, however, much exceed cobra venom in the intensity of their local

| | | |
|---|---|---|
| * Diamantina venom, | 1½ | milligramme. |
| *Sepedon hæmachates*, | 2½ | ,, |
| *Crotalus horridus*, | 4 | ,, |

action. When death is produced by Crotalus venom, the subcutaneous tissues become extensively infiltrated with a large quantity of blood and of blood-stained serum, the underlying muscles are reduced to an almost pulpy blood-stained substance, and post-mortem decomposition occurs very soon after death. Similar changes in the subcutaneous tissues, but to a rather less degree, are caused by the Diamantina venom, and in addition, hæmaturia, or more probably hæmoglobinuria, was invariably produced by lethal and even by large non-lethal doses. I mention these circumstances to indicate the perfection of the protection which is produced by the administration of successive gradually increasing doses; for they can be so adjusted that a dose of each venom, even six times larger than the minimum-lethal, may be administered without producing more than an inconsiderable and often scarcely observable degree of local destructive effect.

In the meantime, the process of protection against the latter venoms has not advanced further than six times the minimum-lethal dose. This, however, has been sufficient to allow experiments to be made by which it has been demonstrated that when an animal has acquired a resistant power over more than the minimum-lethal dose of one venom, that animal is also able successfully to resist the lethal action of a dose above the minimum-lethal of other venoms. To a rabbit protected against cobra venom, a dose above the minimum-lethal of Sepedon venom has been administered; to rabbits protected against Crotalus venom, doses above the minimum-lethal of Diamantina and of cobra venoms have been given; to rabbits protected against the Diamantina venom, doses above the minimum-lethal of Crotalus and Sepedon venoms have been given, and in each case the animal has recovered, and but few symptoms of injury were produced. At the same time, in other experiments, evidence was obtained that animals protected against a given venom are capable of resisting the toxic effect of that venom more effectually than the toxic effects of other venoms.

My experiments have not yet proceeded sufficiently far to show for what length of time the protection conferred by any final lethal dose may last. I propose to make some experiments which will give definite information in regard to this point, which may possibly lead to practical applications. It has incidentally been discovered, however, that protection lasts for at least a considerable period of time, even when the last protective dose has not been a large one. For example, to a rabbit which had last received twice the minimum-lethal dose of Crotalus venom, the same dose was administered twenty days subsequently, and it altogether failed to produce any toxic symptoms.

Before passing to the next part of my communication, it may be stated that as yet no sufficient data have been obtained for affording an explanation of these remarkable facts. It is obvious that the blood of protected animals must contain some substance or substances which are not present in the non-protected animals, by which the lethal and toxic effects of venoms are prevented. I have observed that when the blood-serum of protected animals is added to a solution of venom, a distinctly observable reaction occurs, and this reaction may be of significance when considered along with circumstances, which will be stated in the remaining part of this communication, and especially with the circumstance that the blood-serum itself possesses but little physiological activity. This protective substance may be produced in the body by the influence of the venom, but it is also conceivable that the substance is actually a part of the venom itself, which gradually accumulates under repeated administrations, whereas the lethal and toxic constituents of the venom are more rapidly destroyed or eliminated.

Having thus succeeded in producing a high degree of protection in animals against the toxic effects of serpents' venom, the blood-serum of these animals was, in the next place, collected for the purpose of testing its antidotal properties. In this portion of the investigation, the method followed was essentially the same as that described in a communication made by me to this Society in 1871, on "The Antagonism between the Actions of Physostigma and Atropia," as it appeared to be the most direct method for obtaining accurate knowledge of the value of an antidote.

A few preliminary experiments were early made with the serum of animals in whom the protection had not been carried to a high degree, and they were sufficient to show that antidotal properties are possessed even by this serum. It soon became apparent that in order to obtain some reasonable approximation to constancy in the conditions of the experiments, it was necessary that the serum should be in such a state that it would remain unchanged during at least several weeks. It was found that this could be insured without any appreciable loss of antidotal power by drying the freshly separated serum in the receiver of an air-pump, over sulphuric acid, after it had been passed through a Chamberland's filter. A perfectly dry and easily pulverisable solid was thus obtained, which may be kept unchanged for probably an indefinite time, and from which a normal serum can readily be prepared as required, by merely dissolving a definite quantity of the dry serum in a definite quantity of water.

To this serum, whether in the dry form or in solution, it would be convenient to apply the name "*antivenene*."

The experiments now to be described were made with antivenene derived from the mixed serum of three rabbits, which had last received a dose of cobra venom equivalent to thirty times the minimum-lethal. I avoid the expression "immunized against" thirty times the minimum-lethal dose, for, as a matter of fact, an animal is always protected, or immunized, against a dose considerably above the last which it had received.

The experiments were so planned as to obtain, in three or four different conditions, as exact a definition as possible of the antidotal power of the antivenene. In the meantime, four series of experiments have been undertaken on rabbits. In one series, the venom was mixed outside of the body with the antivenene, and immediately thereafter the mixture was injected under the skin of the animal; in the second series, the venom and antivenene were almost simultaneously injected into opposite sides of the body; in the third series, the antivenene was injected some considerable time before the venom; and in the fourth series, the venom was first injected, and thirty minutes afterwards the antivenene. In the experiments of the third and fourth series, also, the venom and antivenene were injected under the skin of opposite sides of the body.

All, or nearly all, the experiments required to define the exact quantity of antivenene that is sufficient to prevent death from different lethal doses of venom have as yet been made only in the first and fourth of these series. They are, however, in some respects the most important of the series: as the conditions for exactitude in simultaneous administration are perfectly obtained in the first series, and it, therefore, should constitute the basis for comparison between antivenenes derived from different sources; and as upon the results of the fourth series must depend the actual practical application of antivenene to the treatment of poisoning by serpents' bites.

In the experiments of the *first series*, the doses of cobra venom administered were the minimum-lethal, twice the minimum-lethal, thrice the minimum lethal, and four times the minimum-lethal. In the case of each dose, experiments were made with different quantities of antivenene so as to determine the smallest quantity required to prevent death. In order to render it certain, in this and the other series, that a lethal dose was always administered in the experiments with the so-called minimum-lethal, the minimum-lethal indicated by the previous experiments was not used, but instead of it a slightly larger dose (·00026 instead of ·000245 grm. per kilogramme).

When this certainly lethal dose, capable of causing death in five or six hours, was mixed with antivenene and the mixture then injected under the skin, it was found that so small quantities of antivenene were sufficient to prevent death, as ·5 c.c., ·25, ·1, ·05, ·02, ·01, ·005, ·004 c.c. ($\frac{1}{2}$, $\frac{1}{5}$, $\frac{1}{10}$, $\frac{1}{20}$, $\frac{1}{100}$, $\frac{1}{200}$, $\frac{1}{250}$ of a c.c.) for each kilogramme of the weight of the animal. With ·0025 c.c. ($\frac{1}{400}$), however, the animal died. The antivenene was therefore found to be so powerful as an antidote, in the conditions of these experiments, that even the $\frac{1}{250}$ part of a cubic centimetre, equivalent to about one $\frac{1}{15}$ part of a minim, acted as an efficient antidote. Even with the smaller of these doses of antivenene, there was almost no symptom of poisoning produced. In the experiments of this series with twice the minimum-lethal dose, recovery occurred when the doses of antivenene were ·75 c.c., ·7 c.c. and ·6 c.c. per kilogram, but ·5 c.c. per kilogram failed to prevent death. In the experiments with thrice the minimum-lethal dose of venom (a dose capable of producing death in less than two hours), recovery occurred when the doses of antivenene were 1·5 c.c. and 1 c.c., but death occurred with ·8 c.c. And even the enormous dose of four times the minimum-lethal failed to produce death, or indeed any observable disturbance, when it had previously been mixed with 2 c.c. of antivenene for each kilogramme of animal.

In the *second* series, experiments have been made only with twice the minimum-lethal dose of venom. When this dose was injected into the subcutaneous tissue of one side of the body, and immediately thereafter a dose of antivenene, it was found that doses of 1 c.c., 2 c.c. and 3 c.c. per kilogramme failed to prevent death, but that 4 c.c. and 5 c.c. per kilogramme were able to do so.

In the *third* series, the experiments have as yet been made with only the minimum-lethal of cobra venom, and they show that ·4 c.c. per kilogramme of this antivenene is able to prevent death, when given thirty minutes before the venom.

In the *fourth* series, where the results are likely to give the clearest indications of the antidotal value of antivenene, it was found that recovery occurred in the experiments in which 1·5 c.c., 1 c.c., and ·8 c.c. per kilogramme of antivenene were injected *thirty minutes after* a *certain* minimum-lethal dose of venom, but that the antivenene was insufficient in quantity to prevent death when ·75 c.c. per kilogramme or less was administered. In this series, further, it was found that 5 c.c. per kilogramme of antivenene was a sufficient dose to prevent death after twice the minimum-lethal dose of venom; but that 2 c.c., 2·5 c.c., 3 c.c., and 4 c.c. per kilogramme were insufficient.

The experiments of this series are especially interesting, as nearly all the animals showed symptoms of poisoning before the antivenene had been administered. Even in the fatal experiments, the duration of life was greatly prolonged by the administration of antivenene; and it is probable that in many instances a second injection of antivenene, made half-an-hour or an hour after the first, would have prevented death.

It has thus been established, on the clearest evidence, that the blood-serum (antivenene) of animals protected against large lethal doses of venom is able, in varying conditions of administration, perfectly to prevent lethal doses of the venom of the most poisonous of serpents from producing death in non-protected animals.

In order to obtain some evidence bearing on the question as to whether the more powerful antivenene is produced by the long continued administration of small non-lethal doses of venom, or by the administration of doses gradually increasing until a large lethal dose is reached, a few experiments were made with the serum of a rabbit which had received one-tenth part of the minimum-lethal dose nearly every two days during a period of three months and one week, and also of one which had received the one-fourth part of the minimum-lethal dose nearly every four days during a period of three months and three weeks. I did not find that the antidotal power of the antivenenes obtained from these animals was great, or nearly so effective as the antivenenes obtained from animals which had finally received a dose much in excess of the minimum-lethal. When mixed with venom and then injected, 3 c.c. per kilogramme of these antivenenes were insufficient to prevent death from somewhat more than the minimum-lethal dose of venom, but 5 c.c. per kilogramme were sufficient to do so.

I have also administered 1·5 c.c. per kilogramme of cobra antivenene thirty minutes after a dose one-twelfth larger than the minimum lethal of the venoms, respectively, of the *Sepedon hæmachates*, the *Crotalus horridus*, and the *Diamantina* serpent; and the rabbits experimented on have recovered. This successful result is all the more remarkable when the intensely destructive local effects of each, but especially of two, of these venoms is recollected.

The experiments establishing, and to some extent defining, the antidotal power of cobra venom, further, have been made on animals peculiarly susceptible to the poisonous action of serpents' venom, a circumstance of importance in considering the probable value of the antivenene when used as an antidote in the treatment of animals of less susceptibility, among whom there appears to be sufficient evidence to place human beings. The minimum-lethal dose for man probably approximates that of the cat, rather than that of vegetable feeders such as the rabbit, guinea-pig, and white rat.

It is also to be remembered that, in the meantime, the experiments have been restricted to a definition of the antidotal power in certain rigidly adhered to conditions, which were not always the most favourable for the mere prevention of death. Indications have indeed been obtained which render it highly probable that death may be prevented from occurring more certainly by several administrations, rather than by one administration of antivenene, and also by the introduction of the antivenene into the same parts as the venom, rather than into distant parts.

It would be important also to increase the number of the experiments with the larger of the lethal doses of venom as yet administered, and, it may be, to employ still larger doses; although, for practical application, the larger of the doses that have already been used, as they produce death in about an hour, need not be increased.

To these purposes I hope to apply the antivenene soon to be prepared from the rabbits which have already received fifty times the minimum-lethal dose of venom.

For the actual application of the antivenene to the treatment of snake-poisoning in man, an endeavour is being made to obtain the large quantity that is requisite, from a horse now receiving considerable lethal doses of cobra venom. From this source, also, it is hoped that a sufficient quantity will be obtained to allow of the examination of the chemical properties of the antivenene to be continued, with the object of discovering the constituent or constituents by which the antidotal effects are produced. If the isolation of the antidotal constituent or constituents can be effected, an antivenene of greatly increased power will be obtained, and the range of efficient application will be increased. For these objects, however, it will be necessary to administer to the horse much larger doses than it has yet received; and the chief difficulty in doing this is to obtain a sufficient supply of cobra venom. By the great kindness of Surgeon-Colonel Cunningham, 9 grms. of dry venom have already been obtained, but in order to carry the protection to fifty times the minimum-lethal dose, other 30 grms. would be required. I have reason to hope that the India Office will succeed im making arrangements for procuring even this large quantity.

The subject is one of practical importance to India, where the destruction of human life by venomous serpents is represented by an annual mortality of 20,000, and where the failure of all methods of treatment* has led to the introduction of a system of extermination of venomous serpents—apparently futile in its results—in the carrying out of which large sums of money have been expended.

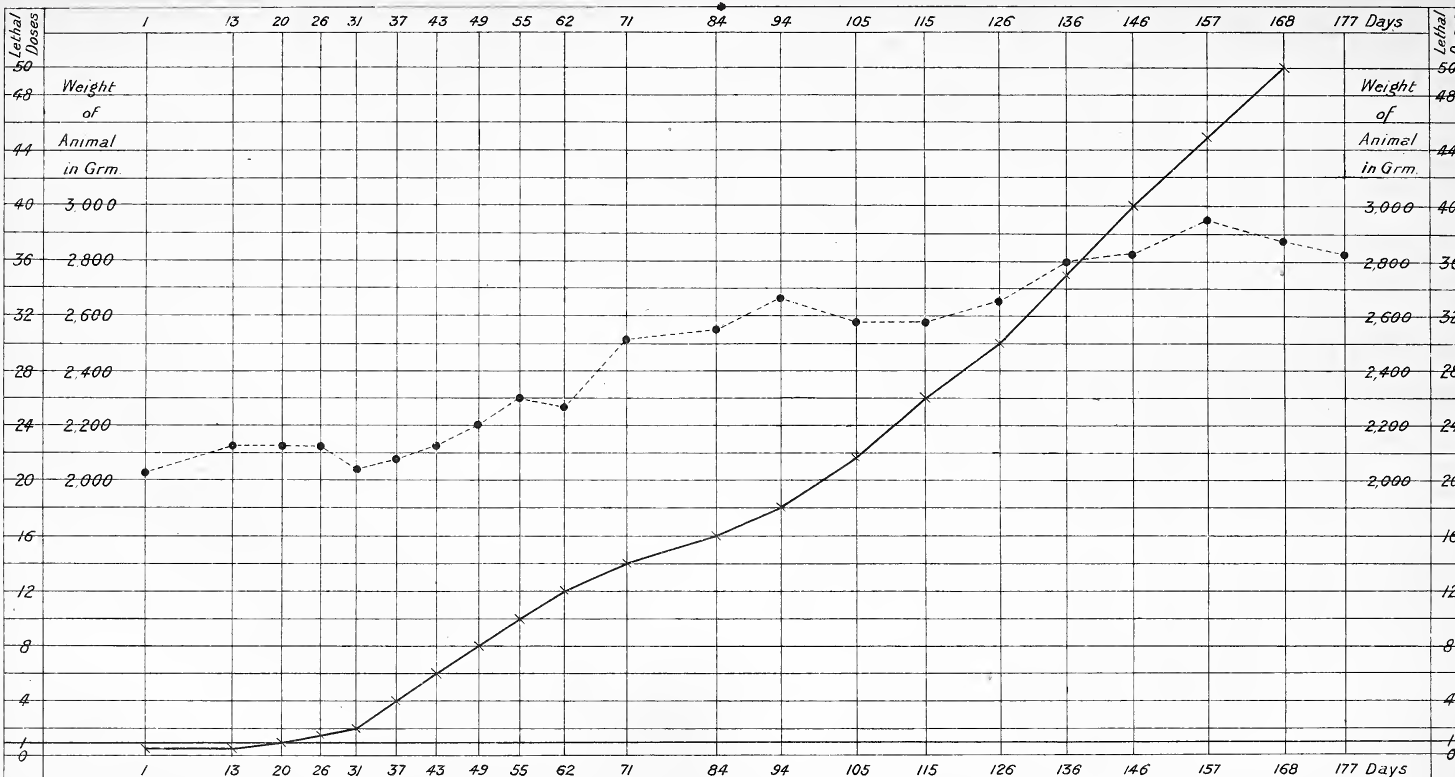
In considering the probabilities of success by antivenene treat-

* "After long and repeated observation in India, and subsequently in England, I am forced to the conclusion that all the remedies hitherto regarded as antidotes are absolutely without any specific effect on the condition produced by the poison."—Sir Joseph Fayrer *On the Nature of Snake Poison.*

ment, it is also to be recollected that antivenene can be obtained even more powerful than that which was used in the experiments which have been described; and that, judging from the statistics of Fayrer and Wall, in 75 per cent. of fatal cases in man death does not occur until from three to twenty-four hours after the infliction of the bite. This latter fact appears to indicate that in the great majority of the fatal cases the dose of venom does not much exceed the actual minimum-lethal; and, therefore, is not so large as the doses whose lethal action has been prevented from occurring in the experiments that have been described; in which, further, the conditions for success in preventing death were not the most favourable that could have been adopted.

It appears to me, however, that an interest and importance as great as can be derived from this practical application of the facts which I have brought before the Society, are to be found in their relation to the cause and treatment of many of the most fatal of diseases—those, namely, which are produced by organisms that have found their way into the body. The evidence in favour of the curative value of the antitoxines derived from animals immunized against the toxines of these diseases, seems to receive an additional confirmation from these facts. They also bring distinctly before us the circumstance that there are limits to this curative power, dependent on the dose of the toxine to be counteracted, on the special antidotal activity of the antitoxine that is used, and on the duration of the time during which the toxine has had an opportunity of exerting its poisonous action before the antitoxine is administered. If these and other conditions interfering with successful treatment are not determined and recognised, unmerited discredit is likely to be attached to remedies which alone of all remedies may be capable of preventing death in these diseases, by counteracting the effects of minimum-lethal and larger doses of the toxine.

IMMUNIZATION OF A RABBIT AGAINST 50 TIMES THE MINIMUM-LETHAL DOSE OF COBRA VENOM.

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The Crosses connected by the Continuous Line represent Administrations of Venom.

The Dots connected by the Interrupted Line represent the Weights of the Animal.

# Further Observations on Antivenene, and on the Production of Immunity against Serpents' Venom; with an Account of the Antidotal Properties of the Blood-Serum of Venomous Serpents. By Professor Fraser, M.D., F.R.S.

(Read July 15, 1895.)

(*Abstract.*)

At the time when my former paper was communicated to the Society, I was engaged in investigating several subjects closely related to those dealt with in that paper, but in regard to which the experimental work had not advanced sufficiently to allow definite statements to be made.

I propose, to-night, to make some statements on these subjects.

*Antivenene of Rabbits protected against fifty times the minimum-lethal dose.*—A description has already been given of the steps by which protection against fifty times the minimum-lethal dose of cobra venom had been produced in rabbits. The antidotal power of the antivenene derived from these rabbits has now been examined. When this antivenene was mixed with twice the minimum-lethal dose of cobra venom and the mixture injected under the skin of a rabbit, it was found that recovery occurred if the dose of antivenene was ·7 c.c. or ·6 c.c., but that the animal died if the dose was ·5 c.c. or ·4 c.c. per kilogramme. As ·65 c.c. per kilogramme of the antivenene derived from rabbits which had last received thirty times the minimum-lethal dose is able to prevent death when mixed with the same lethal dose of venom, this result is an unexpected one. It appears to show that the blood-serum of a rabbit which had last received thirty times the minimum-lethal dose of venom is almost as powerful as an antidote as the blood-serum of a rabbit which had last received fifty times the minimum-lethal dose. If this be the case, it is suggested that for any given species of animal there is a maximum limit to the quantity of antivenene which can be produced or retained in the blood, and that, in the case of the rabbit, this maximum limit is reached when the dose is thirty times the minimum-lethal or even somewhat less. It is also suggested that this maximum-limit is reached before the maximum protection of the animal has been produced; for, undoubtedly, an animal which had last received fifty times the minimum-lethal dose will survive a larger subsequent dose of venom than an animal which had last received only thirty times the minimum-lethal dose. It is probable, therefore, that protection depends not only on the presence in the body of an antidotal substance, but also on a modification in the reaction of the tissues, produced by frequently repeated administrations, which lessen the susceptibility of the tissues to the injurious action of the venom.

*Antivenene derived from the Horse.*—Since my former communication, also, the blood-serum of the horse, then referred to, has been examined. The process of protection had been begun in February with one-fifth the minimum-lethal dose, estimated from the results obtained in other herbivorous animals. This dose was repeated in seven days, and again in five days. One-third the estimated minimum-lethal dose was next administered, then, on two occasions, one-half, then three-fourths, and then the actual minimum-lethal dose. By successive increments, the subcutaneous injections were continued until fifteen times the minimum-lethal was administered, four months and a half after the protecting process had been commenced.

Distinct general disturbance, including a rise of temperature, was produced by the earlier doses. The later and larger doses, however, have produced almost no general reaction, although both the earlier and the later doses have caused considerable local effects, and, conspicuously, subcutaneous œdema and necrosis of portions of the skin.

Ten days after fifteen times the estimated minimum-lethal dose had been administered, blood was taken, with careful antiseptic precautions, from the left jugular vein, and a considerable quantity of serum has thus been obtained. A small portion of this serum was preserved in the liquid state, but the greater part was dried *in vacuo* over sulphuric acid. It yielded 11·5 per cent. of solids in the form of a brittle substance, which was easily broken into bright, transparent, orange-yellow fragments.

The antidotal properties of this serum have been examined in two series of experiments. In the first, the serum, or antivenene, was mixed with cobra venom outside of the body; and in the second, cobra venom was injected thirty minutes before the serum.

In the former series of experiments, it was found that ·005 c.c.,

·004 c.c., ·003 c.c., ·002 c.c., and ·001 c.c. per kilogramme were each sufficient to prevent death from somewhat more than a minimum-lethal dose of venom, but that ·0005 c.c. ($\frac{1}{2000}$) was insufficient.

As the antivenene obtained from protected rabbits which had last received thirty times the minimum-lethal dose failed to prevent death, in the same conditions of experiment, when its dose was ·0025 c.c. ($\frac{1}{400}$th of a c.c.), the antivenene obtained from the horse is about twice as powerful as an antidote as the antivenene of rabbits protected against thirty and even fifty times the minimum-lethal dose.

When given thirty minutes after the same lethal dose of venom, this serum, further, was able to prevent death when the quantity injected was only ·5 c.c. per kilo.

In considering these results it must be recollected that the minimum-lethal dose of venom for horses has not yet been defined, and that no other data are available for forming an estimate than those derived from the determinations described in the former communication, which have been made in a few herbivorous animals. The dose last given to the horse may, therefore, have been considerably more than fifteen times the minimum-lethal dose. On the other hand, it may be the case that the maximum production or retention of antivenene occurs in the horse and other herbivorous animals with fifteen times the minimum-lethal dose, or, to use a chemical phrase, that with this dose the saturation-point of the blood has been reached.

Several interesting and practically important subjects for investigation are thus suggested, with regard to which information is likely to be obtained by an examination of the antivenene of the horse now undergoing protection, after the administration of the larger doses of venom which it is intended should be given.

Although it is certainly desirable that a still more powerful antivenene should be obtained, the antivenene already obtained is of sufficient antidotal power to be applied to the treatment of snake-bite in man; and I propose to send the greater part of it to India for this purpose.

For practical use, it is obvious that the antivenene in the dry state has advantages over a liquid preparation—in respect, for example, to portability, resistance to decomposition, and facility of subdivision into doses.

Some of these advantages are apparent when the specimens, now exhibited, of 15 c.c. of liquid antivenene are compared with the specimens of dry antivenene representing 15 c.c. of serum.

The facts which I had previously communicated to the Society show that the dry antivenene retains the original antidotal power of the liquid serum.

With this antivenene I have also made an experiment which illustrates its value when used as an antidote in actual practice, rather than when used merely for the purpose of defining its antidotal power in the rigidly adhered-to conditions of the experiments which have been described. ·5 c.c. per kilogramme having been found to be about the smallest quantity that can prevent death when given thirty minutes after rather more than the minimum-lethal dose of venom, this dose of venom was administered to a rabbit, and thirty minutes afterwards the insufficient dose of ·4 c.c. per kilogramme of antivenene. In three hours, the animal was lying extended with the head resting on the floor, limp and unable to stand; the respirations were infrequent and shallow; the cardiac action was feeble and irregular; and rattling sounds were being produced in the throat, from the excessive salivary and bronchial secretions always caused by toxic doses of cobra venom. A second dose, consisting of ·6 c.c. of antivenene, was now injected under the skin; and very soon a marked improvement occurred in the condition of the animal, the respirations becoming deeper, and the cardiac action stronger and more rapid, and without irregularity. An hour subsequently, a third dose of antivenene, consisting of ·5 c.c. per kilogramme, was injected; and further improvement was produced, so that all toxic symptoms soon disappeared, and the animal was restored to a nearly normal state, from which no relapse occurred until perfect recovery had become established.

*Influence of Diet in modifying the Minimum-Lethal Dose.*—I have already drawn attention to the remarkable difference in the minimum-lethal dose of venom for herbivorous as contrasted with carnivorous animals. If this difference be due, in any important degree, to the effects, transmitted and individual, of the special diet of each of these two groups of animals, it seemed probable that the minimum-lethal dose might be modified by changing the diet of any animal in whom this could be done without much deterioration of health; for example, by restricting the diet of a herbivorous animal to animal food.

A number of young white rats, accordingly, were put on an animal dietary, as soon as they had been weaned; and, with the slight addition of a little vegetable food once or twice a week, found necessary to maintain them in fairly good health, this dietary was continued for seven weeks. To one of the rats, a dose of cobra venom one-and-a-half times greater than the minimum-lethal was then administered by subcutaneous injection, and, although marked symptoms of poisoning were produced, the rat recovered. Two weeks subsequently, the animal dietary having been continued, another of these white rats received twice the minimum-lethal dose, and it also recovered after a temporary illness. The experiments could not be carried further, as the other members of this family had fallen into bad health, and one after the other had died before this time.

In animals whose progenitors had subsisted mainly upon a vegetable diet, the conversion of the diet into that of carnivorous animals is, therefore, alone sufficient to reduce the vulnerability to venom, and to cause, in this respect, an approximation to the resistance of a carnivorous animal.

This fact appears to indicate that the toxic effects of serpents' venom are dependent to a large extent upon an influence on the blood, an influence as yet only partially and imperfectly recognised.

*Protection produced by Stomach Administration.*—In the experiments which I have hitherto described, and, indeed, apparently in all others made in this new subject of Serum Therapeutics, protection has been produced by the subcutaneous or, less frequently, the intra-venous injection of the venom or other toxic substance.

These methods of administration are attended with inconveniences, which, it seemed possible, might be avoided were the toxic substance introduced into the stomach or other part of the alimentary canal. No doubt, the probability of thus producing protection is opposed by the fact, recognised even at the time of Celsus, and corroborated by such modern observers as Laçerda, Weir Mitchell, Fayrer and Brunton, and Calmette, that serpents' venom is either altogether inert, or nearly so, when it is introduced into the stomach or any other part of the alimentary canal.

Even assuming that venom so introduced is inert, or nearly so, as a poison, it does not necessarily follow that it is incapable of producing protection ; for this protection is, in part at least, dependent on the presence in the blood of a substance or substances which possess no distinct toxic action, and which may therefore be present in the blood as a result of the administration of venom, even although the venom did not produce any evident poisonous symptoms.

In order to obtain some evidence on this subject, the process for producing protection already described was applied to a cat, with the modification that the doses of venom were introduced into the stomach instead of being injected under the skin.

Taking as a basis the minimum-lethal dose by the latter method of administration, the cat received at intervals of from two to five days, one-fifth of the minimum-lethal dose on eight occasions, then one-fourth, and one-third ; and at longer intervals, the minimum-lethal, twice, four times, six times, eight times, ten times, and so on, until, on the 116th day, a dose eighty times larger than the minimum-lethal was introduced into the stomach.

No observable disturbance was produced by any of these doses.

As in further administrations, doses of upwards of a gramme of dry venom would have been required, the experiment was not continued beyond this point, for such large quantities would have soon exhausted the rapidly-diminishing supply of venom.

Eight days after the animal had received by the stomach a dose of venom representing eighty times the minimum-lethal if given subcutaneously, a dose of venom corresponding to one and a half times the minimum-lethal was injected under the skin. No obvious general symptoms followed the administration of this dose, but some local œdema and skin necrosis were produced, and the animal has remained in good health until the present time.

During this experiment, an opportunity occurred for obtaining other facts of some interest. It happened that when the administrations of venom were commenced, the animal was already pregnant, and on the 54th day of the experiment two healthy kittens were born. These kittens were fed exclusively on the mother's milk, the mother continuing to receive gradually increasing doses of venom.

One of the kittens, when fifty-seven days old, and when the mother had last received a dose equivalent to thirty times the minimum-lethal if given subcutaneously, received, by subcutaneous injection, twice the minimum-lethal dose of cobra venom ; and only slight symptoms, consisting chiefly of drowsiness and loss of appetite, were produced, from which the kitten completely recovered in a few hours.

The second kitten, when sixty-nine days old, received, also by subcutaneous injection, thrice the minimum-lethal dose ; but the protection produced through the mother's milk was insufficient to antagonise this large dose of venom, and death followed the administration.

Evidence in favour of the production of protection by stomach administration, as well as of the toxic feebleness of venom when given by this channel, has been obtained with white rats also. Single doses, corresponding to 10, 20, 40, 200, 300, 600, and 1000 times the minimum-lethal if given subcutaneously, were given by stomach administration to each of seven different white rats. Sleepiness and loss of appetite, lasting for a day or two, were the only effects produced even by the larger of these enormous quantities, and all the animals entirely recovered.

A further experiment was made on the white rat which had received 1000 times the minimum-lethal dose. Seven days after this dose had been administered, and when the animal was apparently in good health, twice the minimum-lethal dose was injected under the skin. Distinct though not serious toxic symptoms were produced, consisting of sleepiness, anorexia, and increase of salivary and bronchial secretion ; but in less than twenty-four hours these symptoms had disappeared, and the animal was soon afterwards in a perfectly normal state.

It would, therefore, appear that although serpents' venom, even in enormous quantities, fails to produce any toxic effects when introduced into the stomach, it still confers upon the animal a certain and not inconsiderable degree of resistance against the toxic effects of subsequent lethal doses of venom. That it does so by causing an antidotal substance to be present in the blood is also manifest from the result of the experiment on the kitten, which had been fed with milk derived from a parent receiving venom by the stomach.

In circumstances which are no doubt exceptional, some of these results would admit of useful practical application.

They probably also offered an explanation of the protection apparently enjoyed by certain snake-charmers, as well as by individuals who claim to be protected, whether members of special sects or not; for subcutaneous injection is not likely to be the method, and it certainly was not the method several hundreds of years ago, employed for the introduction of the protection-producing venom into their bodies.

*Antidotal Properties of the Blood-Serum of Venomous Serpents.*— The results of these experiments may explain also the clearly-established protection possessed by venomous serpents themselves.

They, as well as other circumstances, render it important to determine whether the blood of venomous serpents contains, as does that of artificially-protected animals, an actual substance possessing antidotal qualities.

In order to arrive at some definite conclusion on this subject, I have made endeavours to obtain living venomous serpents, and also the serum separated from their blood.

Last year, an arrangement was concluded with one of the best known of the importers of wild animals to supply me with living cobras. He, however, has not succeeded in doing so, because of some exceptional difficulties; but, as an alternative, he has recently sent me several living specimens of the Hamadryas (*Ophiophagus elaps*), a serpent of greater size and more aggressive disposition than the cobra, and reputed to be at least as deadly as it.

A few days after their arrival, it was observed that moulting was about to commence; and as the condition of health is deteriorated during this process, blood has not yet been taken from any living Hamadryas. One of them, however, became sickly and died. A short time after its death, the neck blood-vessels were opened, and, as coagulation fortunately had not occurred, a small quantity of blood was collected, from which a little blood-serum afterwards separated. As no liquid venom could be obtained from this Hamadryas, this serum has been tested against cobra venom. Two experiments were made, in which it was mixed with slightly more than the minimum-lethal dose of cobra venom, and the mixture then injected under the skin of rabbits. When the quantity of Hamadryas serum was ·15 c.c. per kilogramme of animal, death was not prevented; but as the animal did not die until more than seven hours, an antidotal effect had apparently been produced by this quantity of serum. In the second experiment, a larger quantity of serum was used—namely, ·25 c.c. per kilogramme, and the result was entirely successful; for not only did the animal survive, but no decided symptoms of poisoning were manifested during the six hours in which the animal remained under nearly continuous observation.

Two experiments were also made in which this antivenene was administered thirty minutes after rather more than the minimum-lethal dose of cobra venom. In the first, the dose of antivenene was ·3 c.c. per kilogramme; but this dose was found to be an insufficient one, for the animal died in four hours. In the second experiment, ·5 c.c. per kilogramme of antivenene was administered, in the same conditions as in the former experiment, and it proved to be a sufficient quantity, for the animal recovered, after manifesting only slight toxic symptoms.

I hope by-and-by to extend these observations with blood-serum and venom, taken in more favourable circumstances, from the other and larger Hamadryas, which are now apparently in a state of excellent health.

It has, however, already been possible to confirm these results with the blood-serum and venom of another species of serpent. Dr Thomas Bancroft, of Brisbane, Australia, has recently sent me the dried blood-serum of three black snakes (*Pseudechis porphyriacus*) of that country, and also some dried venom removed from the poison-glands of the same three serpents.

The venom, as it has reached me, is not a very active one, the minimum-lethal dose for rabbits being between ·003 and ·0035 gramme for each kilogramme of animal. At the same time, although this serpent is a member of the Colubrine family, the irritative effects at the position of injection, and even more so on the kidneys following its absorption, are intense. In all the experiments made with the venom alone, the urine voided within a few hours was of a dark red, almost black colour, and was found to contain a large quantity of hæmoglobin, but no blood-cells.

Although the quantity of dry serum was small, there was sufficient to allow three experiments to be made, for the purpose of determining if it can prevent death from being produced by a lethal dose of venom, when the two are mixed together before administration. In one of these experiments, the dose of serum was 1 c.c., and that of venom ·0035 gramme per kilogramme of animal; in the second, the dose of serum was ·5 c.c., and the dose of venom the same as in the first experiment; and in the third experiment, the dose of serum was 1 c.c., and that of venom ·004 gramme per kilogramme of animal. In each case, the gratifying result was obtained that the animal survived the administration of these lethal doses of venom.

It has thus been shown that venomous serpents themselves possess a definite substance in the blood-serum, which possesses antidotal properties against their own venom and the venom of other species of serpents.

It is probable that the substance is produced from venom shed upon the mouth-surface, and absorbed into the blood from this surface or elsewhere in the alimentary canal, and also from venom absorbed directly into the lymphatics and blood-vessels of the poison-glands. At the same time, the protection which is enjoyed by several species of serpents may also be produced by venom introduced into the body with the venomous snakes on which some of them, and especially the Hamadryas, largely subsist.

The blood-serums of the two species of venomous serpents that have been examined are certainly not so powerfully antivenene as the serum which can be obtained from artificially protected animals. They have, however, been obtained in conditions which are not the most favourable for determining the true value of the blood-serum of serpents. This can probably only be done in the countries in which the serpents are found.

If this natural antivenene be found to be powerful, then a new, and in some respects convenient, source for antivenene will become available; but even if the antidotal power be not so great as that of the serum of artificially protected animals, it is possible that its value may be increased, and a sufficiently powerful antidote obtained, more rapidly than with entirely unprotected animals, by injecting several successive doses of venom into the serpents themselves.

# On the Dorsal Branches of the Cranial and Spinal Nerves of Elasmobranchs.

By J. C. Ewart, M.D., F.R.S., Regius Professor of Natural History, and F. J. Cole, *Zoological Department, University, Edinburgh.*

(Read March 18, 1895.)

## The Cranial Nerves.

I. *The Glossopharyngeal.*—The glossopharyngeal is usually looked upon as the most typical of the cranial nerves. In *Amia*, according to Allis,* it consists of post- and præ-branchial branches, a visceral or pharyngeal branch, and a dorsal branch which takes part in innervating the lateral line system—supplying by a single twig one of the sense organs of the lateral canal, and in addition a row of pit organs. Hitherto a branch of the IX has not been found passing to any of the lateral sense organs in Elasmobranchs. In a paper on the Lateral sense organs of Elasmobranchs,† the IX nerve was traced to the skin over the auditory region, but no branch was found passing to any of the sense organs. It was, however, subsequently found supplying one or more of the sense organs immediately in front of the most anterior organs supplied by the lateralis nerve. Collinge, in a recent paper "On the Sensory Canal System of Fishes,"‡ says:—"Ewart has suggested that possibly the most anterior portion of the lateralis canal may be innervated by nerve fibres from the glossopharyngeal nerve previous to its leaving the cranial cavity. So far, he has failed in *Læmargus* to find any branches which pass to either the sensory or ampullary canals from this nerve. In *Polyodon* I have met with similar results, the anterior portion of the lateral canal being innervated by a branch of the vagus." Recently we have found that, as in *Amia*, the glossopharyngeal sends branches to the lateral sense organs. In a specimen of *Læmargus*, which had

* "Anat. and Devel. of the Lateral Line System in *Amia calva*," *Jour. Morph.*, vol. ii.

† J. C. Ewart, "Sensory Canals of *Læmargus*," *Trans. Roy. Soc. Edin.*, vol. xxxvii. p. 78.

‡ "Sensory Canal System of Fishes" (Ganoids), *Quart. Jour. of Mic. Sci.*, vol. xxxvi. p. 519.

lain for some time in spirit, the dorsal branch of the IX, in addition to sending two twigs to the skin over the auditory capsule, supplied the three sense organs of the lateral line lying immediately posterior to the commissural canal (see fig. 1). This dorsal branch leaves the main trunk anterior or proximal to the ganglion, and, after accompanying the main trunk for a short distance, runs upwards through the cartilage of the auditory capsule. When about half way up it bends slightly inwards and forwards, and, before leaving the capsule,

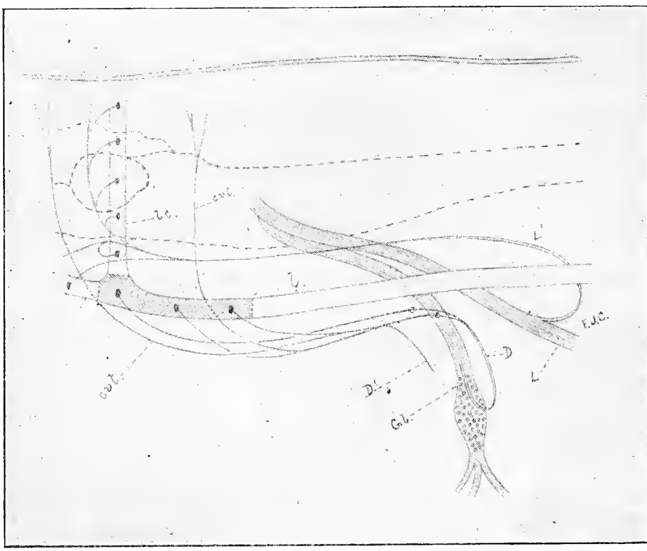


FIG. 1.—Diagram of dorsal branch of IX of *Læmargus microcephalus*. *Gl.*, glossopharyngeal ; *D.*, dorsal branch. Note the 3 twigs to the canal. *l.*, Lateral canal ; *lc.*, commissural (lateral) canal ; *cut.*, cutaneous branches ; *D'.*, branch to inner-postero wall of cranium ; *L.*, lateralis ; *L'.*, branch of lateralis to commissural canal and most anterior portion of lateral canal.

divides into two branches—the slender branch to the most anterior sense organ leaving the capsule by a foramen placed lower than the other and easily escaping notice. A corresponding branch has not yet been found in *Raia* or in any other Elasmobranch. It may be mentioned that in *Amia*, according to Allis (*ibid.*, p. 516), the dorsal branch arises by a separate root, which may pass through the skull by a separate foramen, and has a distinct ganglion ; but no ganglion cells were found in the dorsal branch of *Læmargus*. In

*Læmargus*, as shown in fig. 1, there is a further twig which, running posteriorly, curves round, and is closely applied to the inner-postero wall of the cranium, coursing slightly downwards, and being distributed to the fibrous tissue between the muscles and the cranium.

X. *The Vagus.*—In *Læmargus* the vagus consists of six main trunks, each provided with a ganglion. Of these only one—the lateralis—supplies sense organs. No dorsal branches were observed passing either from the four branchials or from the visceral trunk. On the other hand, the lateralis receives fibres (probably sensory) from the rest of the Xth as it passes through the vagus canal, and the first branch, in addition to innervating the anterior sense organs of the lateral line, sends twigs to the skin immediately behind the auditory capsule. In *Torpedo* the most anterior branch of the lateralis not only springs from the lateralis trunk proximal to its ganglion, but presents a distinct swelling near its origin, in which lie a number of ganglion cells. This is evidently the result of splitting, and resembles the condition of things found in the facial. This branch sends fibres to the skin, and probably also to the sense organs of the lateral canal. In addition to the branch to the skin from the lateralis, the two first branchial nerves apparently sent delicate branches to the skin on a level with their corresponding clefts. These, however, on careful examination were found to arise from the lateralis. It is worth mentioning that each of the four branchial nerves of the *Torpedo* arises by a distinct root, and possesses a well-marked ganglion—there being, as in *Læmargus* and *Raia*, six ganglia in connection with the vagus.

VII. *The Facial.*—The facial used to be described as consisting of two equal portions—the portio dura and the portio mollis or auditory nerve. Gegenbaur on morphological, and Balfour and Marshall on embryological grounds, looked upon the auditory nerve as a dorsal branch of the facial, and not as the equivalent of the portio dura. Others have considered the auditory nerve as of segmental value, for the following reasons :—(1) because of the presence of a ganglion ; (2) the supposed existence of a cleft between the spiracular and first branchial clefts ; and (3) because it develops like an ordinary segmental nerve. If, as is probable, the auditory organ is derived from a lateral sense organ, the auditory nerve will correspond to the dorsal branch of the IX. In all probability further investigation will show that the dorsal branch of the VIIth, by a process of splitting, has given rise in Elasmobranchs to five distinct branches, viz., auditory, superficial ophthalmic, buccal, hyomandibular, and a cutaneous branch, supplying the skin in front of the auditory capsule.

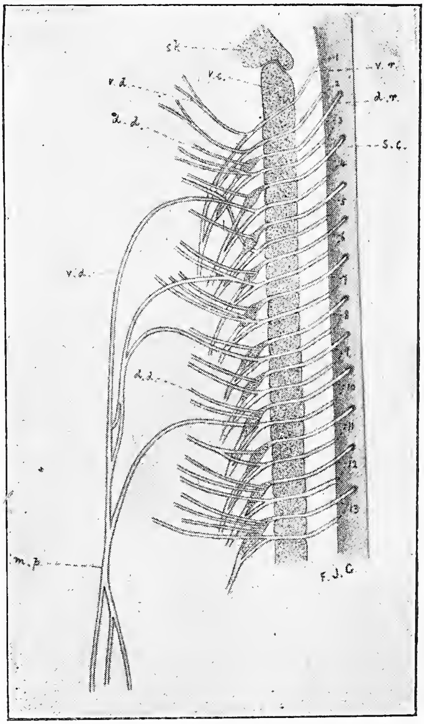



FIG. 2.—Spinal nerves of *Raia batis* dissected with 1″ lens. Semi-diagrammatic. *Sk.*, skull; *v.c.*, vertebral column; *s.c.*, spinal cord; *d.r.*, dorsal root; *v.r.*, ventral root; *d.d.*, dorsal branch from dorsal root (dorsal-dorsal); *v.d.*, dorsal branch from ventral root (ventral-dorsal); *m.p.*, muscular plexus—enters muscle at about level of 20th spinal. This fig. represents the nerves somewhat spread out, but the dorsal roots are really applied to one another, so that the dorsal branches from the ventral root come up between the ganglia on the dorsal roots, and are in all cases closely applied to them. Hence, until dissected, they appear to arise from the dorsal roots.

V. *The Trigeminus.*—The recognised dorsal branches of the Vth are the superficial ophthalmic and the profundus, which supply the skin and gelatinous tissue of the snout. Allis failed to find any branches of the Vth passing to sense organs, but recently Collinge has described an otic branch of the Vth passing to sense organs in *Polyodon*. We have hitherto failed to find, either in *Læmargus* or *Raia*, any branches of the Vth passing to sense organs.

## The Spinal Nerves.

I. *Raia.*—In contrast to other Elasmobranchs, no spinal nerves pass through the cranium in the skate. A typical spinal nerve may be said to have the following dorsal branches (*e.g.*, fig. 2, 8): —(*a*) one from the ventral root (ventral-dorsal), closely applied to, and receiving fibres from, the ganglion on the dorsal root as it comes up, and going with other ventral-dorsals to form a plexus distributed to the muscles applied and dorsal to the vertebral column; (*b*) another from the ganglion on the dorsal root (dorsal-dorsal), not receiving any fibres from the ventral-dorsal, and distributed to the skin. Whilst the greater proportion of the spinal nerves of the skate may be resolved into the above elements, there is still a very considerable amount of variation, and fig. 2 is a semi-diagrammatic representation of the anatomy of the first 13 spinal nerves of a single specimen of *R. batis*, in which the following variations were observed:—(1) ventral-dorsals 1, 2, 3, very much larger than the others. Nos. 1 and 2 are sometimes described as the ventral roots of the vagus, but they are distinctly spinal nerves, though possessing no dorsal roots; (2) ventral-dorsals 3 and 7, immediately after their origin, divide into two large nerves, which do not assist in forming the muscular plexus; (3) ventral-dorsal 5 accompanies dorsal-dorsal 4 instead of 5; (4) ventral-dorsal 6 divides into two branches—one accompanying the dorsal-dorsal, and the other assisting in the formation of the muscular plexus; (5) ventral-dorsal 10 passes through the edge of the ganglion on the dorsal root—(on other specimens it was demonstrated, both by dissections and sections, that the ventral-dorsal passed right through the ganglion, but this is certainly unusual); (6) ventral-dorsal 11 unites with the dorsal-dorsal; (7) ventral-dorsals after 10 were extremely fine, and could only be traced with great care; (8) there were two dorsal-dorsals on 10 and 12, and three on 13.

II. *Læmargus.*—We do not find in Læmargus the same variation in the spinal nerves that is so characteristic of the skate. The ventral roots of the first three pass through the cranium, and, as in the skate, the first two have no dorsal roots, the first again (a very fine nerve) having, apparently, no dorsal branch. The second has a dorsal branch coming up through the cranium to the skin. The third ventral root (the dorsal root of which passes through the vertebral column) has a dorsal branch coming up through the cranium to the skin, but in passing the ganglion on the dorsal root it becomes attached to and receives fibres from it. The anatomy of all the other spinal nerves examined was of the same character; and, it may be mentioned, this origin of the dorsal branch, principally from the ventral root, with fibres from the dorsal root, resembles the condition found in man.

# On the Behaviour of various Alloys in a Steady Magnetic Field. By J. C. Beattie. (With Two Plates.)

(Read July 15, 1895.)

## (*a*) *The transverse effect.*

The direction of the transverse effect in a conductor cannot be given by a knowledge of the magnetic properties of a body, for we find, if we assume the direction to be positive in magnetic bodies, negative in diamagnetic, that nickel, palladium, platinum, manganese, zinc, cadmium, antimony are exceptions. Again, if we look for a relation between the direction of the Thomson effect and that of the transverse effect, we have another long list of exceptions, including antimony, cadmium, zinc, platinum, bismuth. On the other hand, if we arrange the metals in a thermoelectric series, we find that, with the one exception of cobalt, the metals from bismuth to silver have a negative transverse effect; those from zinc to tellurium a positive; and the agreement goes further, for bismuth, which is at one end of the series, has a large transverse negative effect; and as we go up the series, the effect decreases in numerical value; while tellurium, which is at the other end, has a large positive effect, and as we come down the effect again decreases.

If we assume that a relation exists between the transverse effect and thermoelectricity, we should expect that a conductor thermoelectrically negative to bismuth would give a large negative effect; while from a conductor at the other end of the series we should expect a positive effect whose magnitude depended on the position thermoelectrically occupied.

Now, various alloys show peculiarities in their thermoelectric properties. Those of bismuth-antimony are thermoelectrically negative to bismuth, those of antimony-zinc, antimony-cadmium, &c., are positive to antimony. If these give a transverse effect, is its direction and magnitude determined by the thermoelectric position of the alloy considered? The table given below gives the results of experiments made with a number of alloys.

The usual arrangement of the plate for measuring the transverse effect was used. The notation used agrees with that of former papers, only here the total, instead of the half-galvanometer reading of the transverse effect, was divided by the primary reading to obtain the effect per unit current: this multiplied by the thickness of the plate is the number given as transverse effect per unit current per unit thickness; this latter is proportional to the rotatory coefficient R.

The galvanometer used was a d'Arsonval; the uniform field was created by the ring-formed electromagnet previously used.

The plates were protected from air-currents, and, to avoid heating, a pause of several minutes was made between the different observations.

| Alloys arranged in a thermoelectric series. Wiedemann's *Lehre der Electricität*, Bd ii., s. 256–61 (Dritte Auflage). | Sign of effect in component metals. | Effect per unit current per unit thickness of plate for field, 5610 c.g.s. units. |
|---|---|---|
| 19·5 Bismuth, 1 Antimony, | − + | − 1·17 |
| 10 Bismuth, 1 Antimony, | − + | − ·5657 |
| 4 Bismuth, 1 Antimony, | − + | − ·5257 |
| Bismuth (pure), | − | − ·2800 |
| 2 Bismuth, 1 Antimony, | − + | − ·1356 |
| 19·5 Bismuth, 1 Lead, | − 0 | − ·0155 |
| 9·8 Bismuth, 1 Lead, | − 0 | 0 |
| 4·9 Bismuth, 1 Lead, | − 0 | + ·0137 |
| Antimony, | + | + ·01752 |
| 6 Antimony, 1 Zinc, | + + | + ·01438 |
| 806 Antimony, 406 Zinc, 121 Bis., | + + − | + ·1438 |
| 806 Sb., 406 Zn., | + + | + ·1056 |
| Sb., Zn., Cd., | + + + | + ·3960 |
| 806 Sb., 696 Cd., 150 Bis., | + + − | + ·2423 |
| 1 Ab., 1 Cd., | + + | + ·1613 |
| 806 Sb., 696 Cd., | + + | + ·6065 |
| Tellurium, | + | + 2·81 {with 2000 ohms extra inserted in galvanometer. |

If we compare columns (2) and (3), we see that neither the direction nor the magnitude of the transverse effect can be predicted from a knowledge of its value in the metals separately: for example, a small quantity of antimony (+) added to bismuth (−) gives an alloy with an extremely large negative effect (see curve I., fig. 1, diagram I.). Again, an alloy of antimony (+), zinc (+), bismuth (−), has a positive transverse effect nearly ten times larger than in antimony. Antimony and zinc, taken in quantities proportional to their atomic weights, give an effect which is not intermediate in value between those of antimony and zinc separately, but one which is much greater than in antimony; and so on for the other alloys.

Before comparing columns (1) and (3) we must note that the transverse effect is not in all cases a simple one; for example, in the alloys of bismuth and antimony, bismuth-lead, the effect is certainly made up of two parts, one proportional to the first power of a given function, probably the magnetisation, and the other to the third power of the same function. The sign given in the 3rd column refers to the first of these, and we see that it is determined by the alloy's thermoelectric position.

So much cannot, however, be said about the numerical value: various discrepancies are to be found at the positive end of the series, but these are such as could be accounted for by a variation in the thermoelectric position. To answer this question it would be necessary to examine the thermoelectric properties in the different plates used, which, however, was not in this case attempted.

If we consider the above results with those already obtained for other metals and alloys, we arrive at the following result:—*With the exception of cobalt, the simple transverse effect (Hall effect) in conductors has its direction certainly, and its magnitude probably, determined by the thermoelectric power of the conductor considered.*

### (*b*) *The Variation of Resistance.*

The modification of the Wheatstone bridge method, due to Lord Kelvin, was employed to measure the variation. The galvanometer reading for a given field strength—the bridge having been previously arranged so that no current passed through the galvanometer when the field was off—divided by the primary current, was taken as proportional to the variation of resistance. Unfortunately the percentage variation was not determined.

It is well known that in bismuth the variation of resistance is great, and it was found that in the bismuth-antimony alloys the variation was greatest in those alloys which contained the greatest proportion of bismuth ; in the bismuth-lead alloys also the variation decreased as lead was added—(compare fig. 4, diagram II., and fig. 3, diagram I.). In the antimony-cadmium-bismuth alloy a small variation was observed. In the alloy of 6 parts antimony to 1 of zinc a small variation was also observed; but in 806 antimony 406 zinc a variation was not observed, nor could it be observed in the antimony-cadmium alloys.

In all cases the variation was an increase. Only one position of the plate was considered, viz., that in which the plate's length stood perpendicular to the direction of the magnetic field.

The results would seem to indicate that the *variation of resistances of an alloy in a steady magnetic field can be predicted from a knowledge of the resistance variation of the metals composing the alloy.*

### (*c*) *The relation between the Transverse Effect and the Variation of Resistance.*

The equations used were

$$c_1\sqrt{\Delta n} = \pm \mathrm{E}$$

and

$$c_1\sqrt{\Delta n} + c_2(\sqrt{\Delta n})^3 = \pm \mathrm{E}$$

where $\Delta n$ and E are the variation of resistance and the numerical value of the transverse effect respectively as before defined. The relative values of the constants are given in the table of results ; the only peculiarity to be noticed is the negative value of $c_2$ for the alloy 19·5 bismuth 1 antimony ; that is, that part of the transverse effect which is proportional to $(\Delta n)^{\frac{3}{2}}$ has the same sign as the component which is proportional to $(\Delta n)^{\frac{1}{2}}$ ; the two effects working together give the extremely large negative result observed. In the other alloys of bismuth-antimony and in those of bismuth-lead the sign of $c_2$ is positive, and the result is that either the original negative effect becomes positive with higher fields, or at any rate reaches a maximum numerical value.

It is to be noted that the second effect only appears in the bismuth alloys: for example, in tellurium, with an extremely large positive effect, the latter is proportional to $\Delta n^{\frac{1}{2}}$. See "On variation of resistance in Nickel, Antimony, Tellurium."

### (*d*) *Numerical Results and Diagrams.*

Bismuth-antimony alloys. These are easily cast, and the plates can be brought down to almost any desired thinness by simply filing.

## Plate I.

Bismuth, 13·4 grms.; antimony, ·7 grms.

$$\left.\begin{cases} \text{Length:} & 29{\cdot}9 \\ \text{Breadth:} & 17{\cdot}5 \\ \text{Thickness:} & {\cdot}76 \end{cases}\right\} \text{mms.}$$

Temp. 17° C.

$$c_1\sqrt{\Delta n} + c_2(\sqrt{\Delta n})^3 = \text{Trans. effect.}$$

| Field. | Trans. effect. | $\Delta n$ | $\sqrt{\Delta n}$ | $c_1$ | $c_2$ |
|---|---|---|---|---|---|
| 1,360 | − ·4422 | ·0363 | ·1906 | ... | |
| 2,890 | − ·8601 | ·1154 | ·3397 | ... | |
| 5,610 | − 1·5015 | ·2921 | ·5405 | − 2·36 | − 1·4 |
| 11,050 | − 2·3335 | ·5703 | ·7552 | − 2·39 | − 1·2 |
| 14,620 | − 2·8029 | ·7265 | ·8523 | − 2·40 | − 1·2 |
| 15,640 | − 2·9634 | ·7774 | ·8817 | − 2·44 | − 1·2 |
| 17,000 | − 3·0710 | ... | | ... | |

PLATE II.

Bismuth, 8·5 grms.; antimony, ·85 grms. { Length: 23·0; Breadth: 12·0; Thickness: 1·16 } mms.

Temp. 17° C.

| Field. | Trans. effect. | $\Delta n$ | $\sqrt{\Delta n}$ |
|---|---|---|---|
| 2,890 | – ·3206 | ... | |
| 5,610 | – ·4877 | ·2301 | ·4800 |
| 11,050 | – ·6725 | ·4409 | ·6640 |
| 14,620 | – ·7244 | ·5885 | ·7661 |
| 15,640 | – ·7507 | ·6276 | ·7922 |
| 17,000 | – ·7216 | ... | |

In this plate the transverse effect was very unsteady, varying as much as 10 per cent. for the same field strength.

PLATE III.

Bismuth, 8·5 grms. ; antimony, 2·15 grms. { Length: 29·4; Breadth: 11·; Thickness: 1·1 } mms.

Temp. 19° C.

| Field. | Trans. effect. | $\Delta n$ | $\sqrt{\Delta n}$ |
|---|---|---|---|
| 2,890 | – ·2854 | ... | |
| 5,610 | – ·4779 | ·1247 | ·3531 |
| 11,050 | – ·6611 | ·2394 | ·4893 |
| 14,620 | – ·6792 | ·2941 | ·5423 |
| 15,640 | – ·6561 | ·3046 | ·5519 |
| 17,000 | – ·6459 | ·3330 | ·5770 |

In this plate also the transverse effect was very unsteady.

## Plate IV.

Bismuth, 8·5 grms.; antimony, 4·15 grms. { Length: 27, Breadth: 12, Thickness: 1·2 } mms.

Temp. 23° C.

| Field. | Trans. effect. | $\Delta n$ | $\sqrt{\Delta n}$ | $c_1(\Delta n)^{\frac{1}{2}} + c_2(\Delta n)^{\frac{3}{2}}$ = trans. effect. $c_1$ | $c_2$ |
|---|---|---|---|---|---|
| 5,610 | − ·1130 | ·0276 | ·1661 | ... | |
| 11,050 | − ·1633 | ·0606 | ·2461 | − ·69 | + ·51 |
| 14,620 | − ·1896 | ·0846 | ·2909 | − ·69 | + ·50 |
| 15,640 | − ·1949 | ·0911 | ·3018 | − ·69 | + ·53 |
| 17,000 | − ·19 | ... | | ... | |

In diagram I., fig. 1, the field strength in c.g.s. units is measured along the $x$ axis; the transverse effect per unit current along the $y$ axis; curves I, II, III, IV give the relation of the transverse effect to field strength for the plates so numbered. B refers to a plate of pure bismuth. Antimony's corresponding curve would lie on the opposite side of the $x$ axis. Compare with table on page 482, and with remarks on pages 483 and 484.

If fig. 4 be taken as given in natural size, then II, III, B are half natural size, I is $\frac{1}{5}$ natural size.

In diagram II., fig. 4, the relation between the variation of resistance and the field strength for Plates I., II., III., IV. is given. The $x$ axis is the axis of field strength: the $y$ axis that of resistance variation per unit current as given by the galvanometer reading. The scale is the same for all the figures.

It will be noticed that the figure I is similar to the corresponding figure for pure bismuth. Figures II, III, IV, on the other hand, are straight lines.

*Bismuth-Lead Alloys.*—These also are easily cast, and easily worked into a proper shape. It is of advantage to have the plates very thin, as the transverse effect decreases very rapidly as lead is added.

PLATE V.

Bismuth, 19·5 grms.; lead, 0·7 grms. { Length: 30·0; Breadth: 18·0; Thickness: 1·15 } mms.

Temp. 19° C.

| Field. | Trans. effect. | $\Delta n$ | $\sqrt{\Delta n}$ | $c_1\Delta n^{\frac{1}{2}}+c_2\Delta n^{\frac{3}{2}}=$Trans. effect. | |
|---|---|---|---|---|---|
| 1,760 | − ·0229 | ·0157 | ·126 | ... | ... |
| 2,890 | − ·0272 | ·0285 | ·169 | ... | ... |
| 4,400 | − ·0202 | ·0617 | ·248 | − ·211 | +2·1 |
| 5,610 | − ·0135 | ·0727 | ·270 | − ·208 | +2·3 |
| 11,050 | + ·0395 | ·1310 | ·362 | − ·217 | +2·5 |
| 14,620 | + ·0632 | ·1679 | ·409 | − ·215 | +2·2 |
| 15,640 | + ·0672 | ·1769 | ·421 | − ·206 | +2·0 |
| 17,000 | + ·0702 | ·1863 | ·431 | − ·210 | +2·0 |

PLATE VI.

Bismuth, 13·7 grms.; lead, 1·4 grms. { Length: 29·5; Breadth: 16·8; Thickness: ·75 } mms.

Temp. 20° C.

| Field. | Trans. effect. | $\Delta n$ | $\Delta n^{\frac{1}{2}}$ | $c_1\Delta n^{\frac{1}{2}}+c_2\Delta n^{\frac{3}{2}}=$Trans. effect. | |
|---|---|---|---|---|---|
| 2,890 | − ·0038 | ·0120 | ·108 | ... | ... |
| 5,610 | ·0000 | ·0255 | ·160 | − ·071 | +2·8 |
| 11,050 | + ·01648 | ·0488 | ·221 | − ·072 | +3·0 |
| 14,620 | + ·0239 | ·0586 | ·241 | − ·070 | +2·9 |
| 15,640 | + ·0281 | ·0631 | ·251 | − ·070 | +2·9 |
| 17,000 | + ·0301 | ·0671 | ·259 | − ·071 | +2·8 |

With a third alloy—VII. Bismuth, 13 grms.; lead, 2·4 grms.—the transverse effect was observable and was throughout positive. The equation $c_1\Delta n^{\frac{1}{2}} + c_2\Delta n^{\frac{3}{2}}$ = transverse effect only holds if we take a weak field as our starting field; if we take one of the higher ones and combine it with all others, the constants obtained with the high fields are much less than those obtained with low ones. In fact, the transverse effect for high fields is practically proportional to $\Delta n^{\frac{1}{2}}$.

In diagram I., figs. 2 and 3, we have the transverse effect and variation of resistance for a pure bismuth plate—B, fig. 2; and for the alloys V, VI, VII. Compare diagram I., fig. 1, diagram II., fig. 4.

In fig. 3 it will be noticed that V resembles the corresponding bismuth curve, while VI and VII are concave towards the $x$ axis, VII more so than VI.

Curve B in fig. 2, is drawn to scale $\frac{1}{4}$, V and VI to scale unity.

*Antimony-Zinc Alloys.*—Difficult to cast, and difficult to bring into proper form. The plates used were in many cases not quite regular throughout.

PLATE VIII.

Antimony, 18 grms.; zinc, 3 grms. { Length: 25·25; Breadth: 14·75; Thickness: 1·08 } mms.

Temp. 20° C.

| Field. | Trans. effect. | |
|---|---|---|
| 5,610 | + ·0134 | A small increase in resistance observed. |
| 11,050 | + ·0241 | |
| 14,620 | + ·0322 | |
| 15,640 | + ·0347 | |
| 17,000 | + ·0376 | |

PLATE IX.

Antimony, 806 ; zinc, 406 { Length: 30·0 ; Breadth: 9·0 ; Thickness: 3·3 } mms.

| Field. | Trans. effect. | |
|---|---|---|
| 5,610 | + ·0320 | |
| 11,050 | + ·0643 | |
| 14,620 | + ·0855 | |
| 15,640 | + ·09088 | |
| 17,000 | + ·0933 | |

In diagram II., fig. 5, the relation between the transverse effect per unit current and the field for plates VIII. and IX. is shown.

Both figures are drawn to the same scale, and in them the difference of thickness is not taken into account; both are straight lines. *Cf.* Kundt's figures for gold and silver, *Wiedemann's Annalen*, Bd. 49, 1893.

*Antimony-Cadmium Alloys.*—Difficult to work with on account of brittleness.

PLATE X.

1 Antimony, 1 Cadmium { Length: 20·0 ; Breadth: 10·0 ; Thickness: 3·18 } mms.

Temp. 23° C.

| Field. | Trans. effect. | |
|---|---|---|
| 5,610 | + ·0504 | No variation of resistance observed. |
| 11,050 | + ·0957 | |
| 14,620 | + ·1159 | |
| 15,640 | + ·1313 | |
| 17,000 | + ·1405 | |

## Plate XI.

806 Antimony, 696 Cd. { Length: 30·0 ; Breadth: 18·5 ; Thickness: 5·0 } mms.

| Field. | Trans. effect. | |
|---|---|---|
| 5,610 | + ·1213 | No variation of resistance observable. |
| 11,050 | + ·2809 | |
| 14,620 | + ·3623 | |
| 15,640 | + ·3885 | |

Fig. 6, diagram II., gives the relation between transverse effect and field strength. The two curves are again straight lines.

## Plate XII.

Antimony 806, Zinc 406, Bismuth 121 { Length: 33·4 ; Breadth: 16·3 ; Thickness: 2·06 } mms.

| Field. | Trans. effect. | |
|---|---|---|
| 5,610 | + ·0698 | Resistance variation not observable. |
| 11,050 | + ·1373 | |
| 14,620 | + ·1855 | |
| 15,640 | + ·2036 | |
| 17,000 | + ·2154 | |

PLATE XIII.

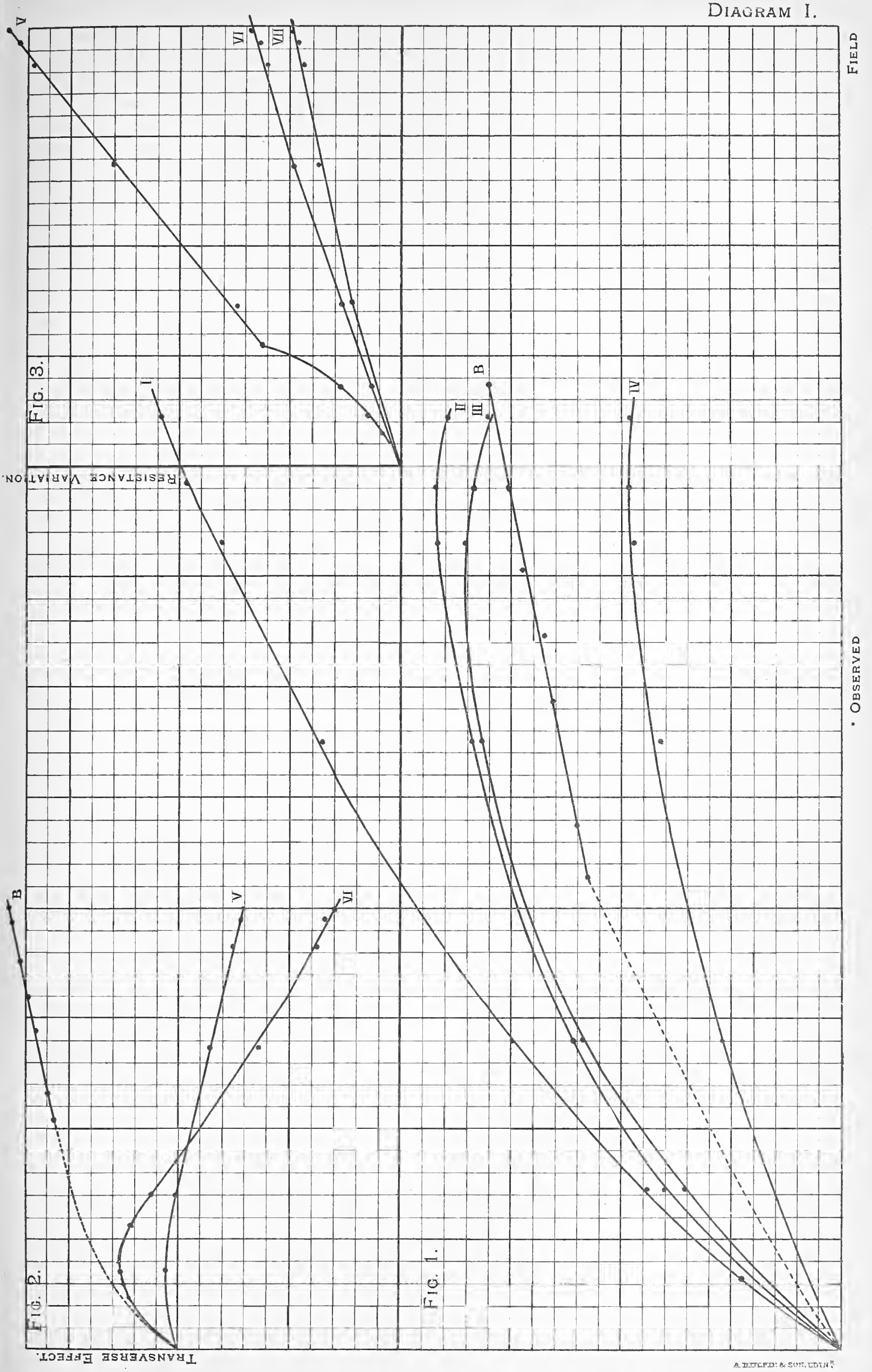
806 Antimony, 696 Cd., 150 Bismuth { Length: 30·0; Breadth: 14·0; Thickness: 2·14 } mms.

| Field. | Trans. effect. | |
|---|---|---|
| 5,610 | + ·0557 | A small increase of resistance was observed. |
| 11,050 | ·1446 | |
| 14,620 | ·2087 | |
| 15,640 | ·2296 | |
| 17,000 | ·2398 | |

Fig. 7, diagram II., gives the relations for plates XII. and XIII.

# On the Behaviour of Various Alloys in a Steady Magnetic Field.

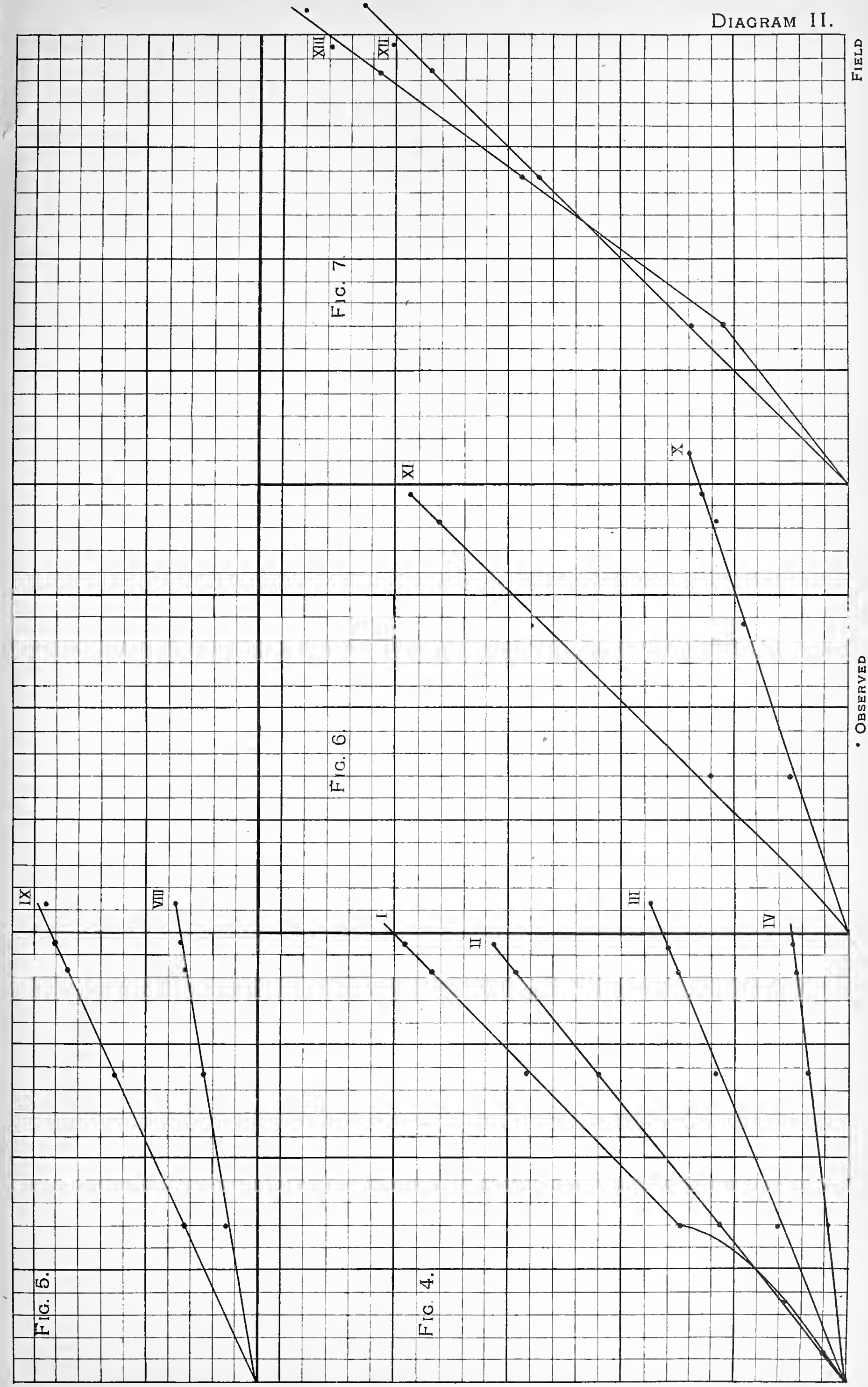
Diagram I.




A. Ritchie & Son, Edinʳ.

# On the Behaviour of Various Alloys in a Steady Magnetic Field.

Diagram II.




A. Ritchie & Son, Edin^r.

# On the Variation of Resistance in a Steady Magnetic Field observed in Nickel, Antimony, and Tellurium Plates. By J. C. Beattie. (With a Plate.)

(Read July 15, 1895.)

Whether the variation of resistance in conductors in a steady magnetic field depends on the field strength, on the magnetisation, or on both, does not seem to be definitely settled. The various experimental methods as yet used are fundamentally similar in principle, and have the disadvantage that the experiments are difficult to perform.

A method where the experimental difficulties are very few is to be found in the comparison of the variation of resistance for any given field-strength with some other phenomenon whose dependence on the field-strength or on the magnetisation is already known, and whose exact determination offers no experimental difficulties. Such a phenomenon is the transverse effect. Kundt has shown that this is in iron, nickel, cobalt, gold, and silver proportional to the magnetisation. In antimony and tellurium, however, we can only infer the relation by a comparison of their curves with those for the above metals, or for bismuth (pure), where the same relation holds.

That for antimony, viz., the curve showing the relation of the transverse effect to the magnetic field, is a straight line agreeing with those for gold and silver; we are justified, therefore, in assuming the transverse effect in antimony to be—as in gold and silver—proportional to the magnetisation. The corresponding curve for tellurium is also approximately a straight line, or, more exactly, two straight lines concave to the field-axis, which again suggests that the transverse effect is proportional to the magnetisation.

On account of the short time at my disposal, I found it impossible to investigate iron and cobalt plates, and in nickel only the variation of resistance perpendicular to the lines of magnetic force was considered. I propose, later, to consider the magnetic metals more thoroughly. In tellurium and antimony the variations of resistance perpendicular to the lines of magnetic force were considered.

The notation is the same as that of my other papers, viz.: by transverse effect is meant the galvanometer reading divided by the primary current; in nickel, however, the readings of four observations are added together.

The variation of resistance was measured by the Wheatstone bridge method, as modified by Lord Kelvin. The galvanometer reading—the bridge being first arranged so as to give no current with the electromagnet off—divided by the primary, was taken as its measure.

The results for the three metals can be summed up as follows :—

*The decrease of resistance in a nickel plate placed perpendicular to the lines of force in a steady magnetic field, and the increase of resistance in plates of antimony and tellurium similarly placed, are proportional to the magnetisation squared.*

*Numerical Results and Diagrams.*

The temperature of the room was about 25° C. when the experiments were made.

Antimony, . . . { Length: 33·0, Breadth: 18·5, Thickness: 1·07 } mms.

I. *Plate cut from a Block of Star Antimony and worked into proper form by filing.*

| Field. | Trans. effect observed. | $\Delta n$ observed. | $(\Delta n)^{\frac{1}{2}}$ | $c_1\sqrt{\Delta n}$ = Trans. effect. $c_1$ |
|---|---|---|---|---|
| 5,610 | + ·0229 | ... | | ... |
| 11,050 | + ·0426 | + ·0408 | ·202 | + ·21 |
| 14,620 | + ·0569 | ·0659 | ·242 | ·23 |
| 15,640 | + ·0601 | ·0745 | ·273 | ·22 |
| 17,000 | + ·0630 | ·0874 | ·295 | ·24 |

II. *Plate cast from same block of Antimony.*

Length: 41·5 }
Breadth: 25·8 } mms.
Thickness: ·4 }

$$c_1 \sqrt{\Delta n} = \mathrm{E}.$$

| Field. | Trans. effect observed. | $\Delta n$ observed. | $\sqrt{\Delta n}$ observed. | $c_1$ | Trans. effect calculated. | $\sqrt{\Delta n}$ calculated. |
|---|---|---|---|---|---|---|
| 5,610 | ·0438 | ... | | ... | ... | ·066 |
| 7,820 | ·0587 | ·0081 | + ·09 | ·652 | + ·0589 | ·088 |
| 11,050 | ·0793 | ·0139 | ·118 | ·672 | + ·0785 | ·119 |
| 13,600 | ·0968 | ·0214 | ·146 | ·663 | ·0971 | ·145 |
| 14,620 | ·1061 | + ·0249 | ·158 | ·671 | ·1050 | ·159 |
| 15,640 | ·1147 | ·0299 | ·173 | ·663 | ·1151 | ·172 |
| 17,000 | ·1189 | ·0311 | ·177 | ·672 | ·1178 | ·178 |

Average ·6655.

*Diagram II., figs.* 12 *and* 11, *shows the relation between Field Strength and Transverse Effect and Field Strength and Variation of Resistance for Plate II.*

Tellurium, . . . { Length: 25·0 ; Breadth: 13·5 ; Thickness: 2·78 } mms.

| Field. | Trans. effect observed. | $\Delta n$ observed. | $\sqrt{\Delta n}$ observed. | $c_1$ | Trans. effect calculated. | $\Delta n^{\frac{1}{2}}$ calculated. |
|---|---|---|---|---|---|---|
| 5,610 | +2·81 | + ·037 | + ·19 | +14·7 | +2·72 | + ·19 |
| 7,820 | 3·85 | ·0726 | ·27 | 14·2 | 3·87 | ·269 |
| 11,050 | 5·30 | ·142 | ·37 | 14·3 | 5·30 | ·37 |
| 13,600 | 6·45 | ·208 | ·46 | 14·0 | 6·59 | ·45 |
| 14,620 | 7·05 | ·248 | ·49 | 14·4 | 7·03 | ·49 |
| 15,640 | 7·64 | ·279 | ·53 | 14·4 | 7·59 | ·53 |

Average 14·3.

The transfer effect in this metal is extremely great; in observing it an extra 2000 ohms had to be inserted in the galvanometer.

IV. Nickel, . . . { Length: 56·0, Breadth: 12·5, Thickness: ·1 } mms.

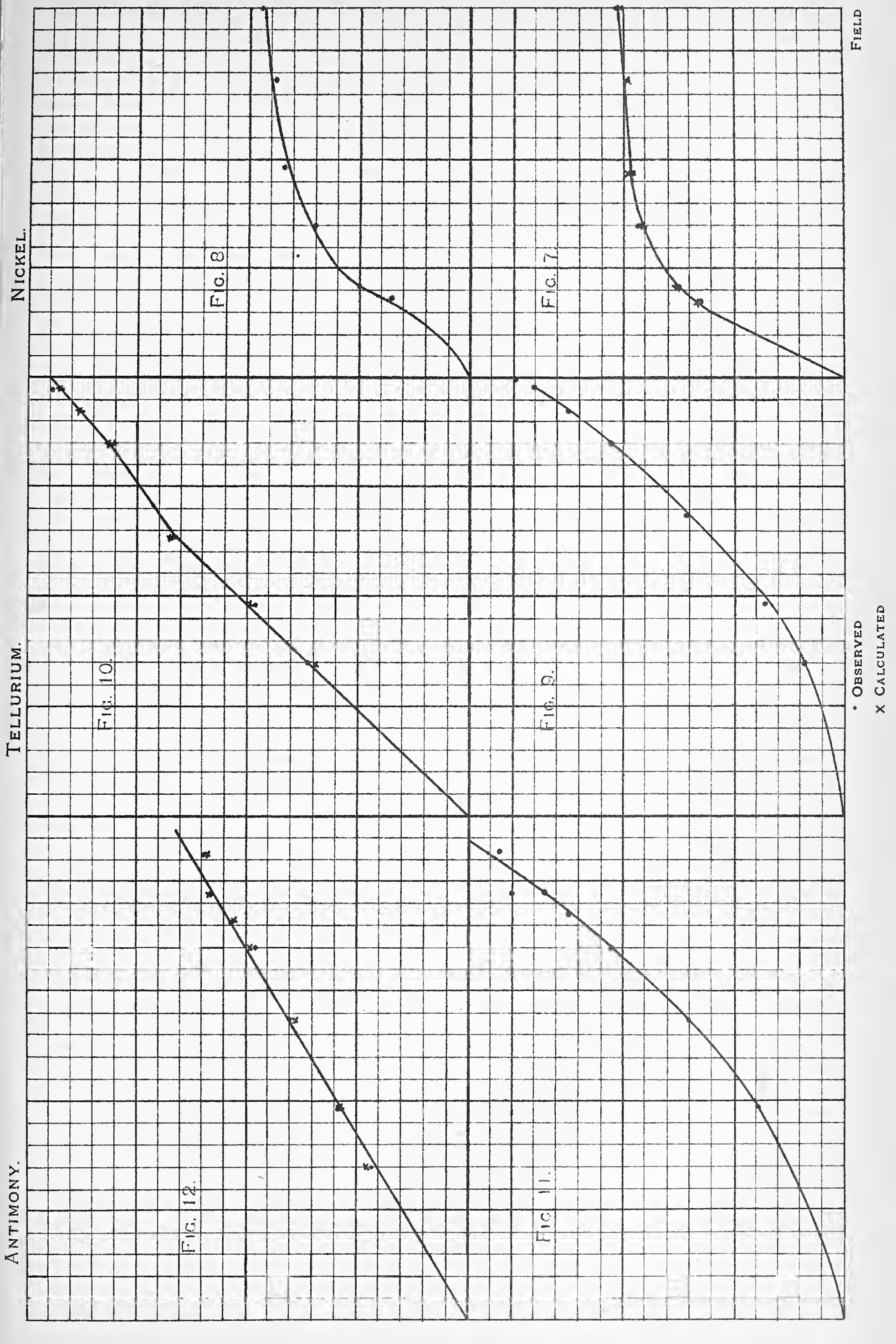
| Field. | Trans. effect observed. | $\Delta n$ observed. | $\sqrt{\Delta n}$ observed. | $c_1$ | Trans. effect calculated. | $\sqrt{\Delta n}$ calculated. |
|---|---|---|---|---|---|---|
| 2,890 | − ·0317 | − ·0037 | − ·0608 | ·52 | − ·0324 | − ·0598 |
| 3,230 | − ·037 | − ·0050 | − ·0707 | ·53 | − ·0377 | − ·0699 |
| 5,610 | − ·0462 | − ·0069 | − ·0839 | ·55 | − ·0449 | − ·0872 |
| 7,820 | − ·0470 | − ·0084 | − ·0911 | ·55 | − ·0485 | − ·0900 |
| 11,050 | − ·0491 | ·0088 | − ·0939 | ·52 | − ·0498 | − ·0926 |
| 13,600 | − ·0518 | ·0094 | − ·0969 | ·53 | − ·0514 | − ·0977 |

Fig. 9, diagram III., gives the relation between Field Strength and Variation of Resistance for Tellurium; fig. 10 that between Transverse Effect and Variation of Resistance.

The corresponding curves for nickel are given in figs. 8 and 7, respectively.

# On the Variation of Resistance in a Steady Magnetic Field.

Diagram III.



## Systems of Plane Curves whose Orthogonals form a Similar System. By Prof. Tait.

(*Abstract.*)

(Read May 6, 1895.)

While tracing the lines of motion and the meridian sections of their orthogonal surfaces for an infinite mass of perfect fluid disturbed by a moving sphere:—the question occurred to me "When are such systems similar?" In the problem alluded to, the equations of the curves are, respectively,

$$(r/a)^2 = \cos\theta\,, \quad \text{and} \quad (r/b)^{\frac{1}{2}} = \sin\theta\,.$$

It was at once obvious that any sets of curves such as

$$(r/a)^m = \cos\theta \quad \text{and} \quad (r/b)^{\frac{1}{m}} = \sin\theta$$

are orthogonals. But they form *similar* systems only when

$$m^2 = 1\,.$$

Hence the only sets of similar orthogonal curves, having equations of the above form, are (*a*) groups of parallel lines and (*b*) their electric images (circles touching each other at one point). As the electric images of these, taken from what point we please, simply reproduce the same system, I fancied at first that the solution must be unique:—and that it would furnish an even more remarkable example of limitation than does the problem of dividing space into infinitesimal cubes. (See *Proc.* vol. xix. p. 193.) But I found that I could not prove this proposition; and I soon fell in with an infinite class of orthogonals having the required property. These are all of the type

$$r\frac{d\theta}{dr} = (\tan\theta)^{2m+1} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1).$$

which includes the straight lines and circles already specified. The next to these in order of simplicity among this class is

$$r = a\epsilon^{\frac{1}{2\cos^2\theta}}\cos\theta\,,$$

with $$r = b\epsilon^{\frac{1}{2\sin^2\theta}}\sin\theta\,.$$

In order to get other solutions from any one pair like this, we must take its electric image from a point whose vector is inclined at $\pi/4$ or $3\pi/4$ to the line of reference. For such points alone make the images similar. And a peculiarity now presents itself, in that the new systems are not directly superposable:—but each is the perversion of the other.

If we had, from the first, contemplated the question from this point of view, an exceedingly simple pair of solutions would have been furnished at once by the obviously orthogonal sets of logarithmic spirals

$$r = a\epsilon^{\theta}, \quad r = b\epsilon^{-\theta};$$

and another by their electric images taken from any point whatever. The groups of curves thus obtained form a curious series of spirals, all but one of each series being a continuous line of finite length whose ends circulate in opposite senses round two poles, and having therefore one point of inflection. The excepted member of each series is of infinite length, having an asymptote in place of the point of inflection. This is in accordance with the facts that:—a point of inflection can occur in the image only when the circle of curvature of the object curve passes through the reflecting centre, and that no two circles of curvature of a logarithmic spiral can meet one another.

We may take the electric images of these, over and over again, provided the reflecting centre be taken always on the line joining the poles. All such images will be cases satisfying the modified form of the problem.

If we now introduce, as a factor of the right hand member of (1), a function of $\theta$ which is changed into its own reciprocal (without change of sign) when $\theta$ increases by $\pi/2$, we may obtain an infinite number of additional classes of solutions of the original question; and from these, by taking their electric images as above, we derive corresponding solutions of the modified form. We may thus obtain an infinite number of classes of solutions where the equations are expressible in ordinary algebraic, not transcendental, forms.

Thus we may take, as a factor in (1), $\tan^2(\theta + \alpha)$. The general integral is complicated, so take the very particular case of $m = 1$, $\alpha = \pi/4$. This gives the curves $r = a\dfrac{\tan\theta\sec\theta}{(1+\tan\theta)^2}\epsilon^{2/(1+\tan\theta)}$. Again, let the factor be $\tan(\theta - \alpha)\tan(\theta + \alpha)$. With $m = 1$, and $\tan\alpha = 1/\sqrt{3}$, we get the remarkably simple form $\dfrac{x}{a} = 1 - \dfrac{y^2}{3x^2}$. But such examples may be multiplied indefinitely.

## Meetings of the Royal Society—Session 1892–93.

*Monday, 28th November* 1892.

General Statutory Meeting. Election of Office-Bearers. *P.* 1.

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*Monday, 5th December* 1892.

Sir Douglas Maclagan, M.D., President, in the Chair.

The Chairman gave an Introductory Address. *P.* 2.

The Hon. Lord M'Laren, Vice-President, presented, in name of the Committee of Subscribers, Sir George Reid's portrait of the General Secretary.

The following Communications were read:—

1. Note on Uniform Convergency of Series. By Professor Cayley. *P.* xix. 203.
2. Note on a certain Locus. By Professor P. H. Schoute, of Groningen. Communicated by Professor Tait. *P.* xix. 208.
3. On the Division of Space into Cubes. By Professor Tait. *P.* xix. 193.

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*Monday, 19th December* 1892.

Sir Douglas Maclagan, M.D., President, in the Chair.

The following Communications were read:—

1. Obituary Notice of the late Thomas Nelson. By W. Scott Dalgleish, Esq., M.A., LL.D. *P.* xix. lviii.
2. On a further Development of Kjeldahl's Method of Organic Analysis. By Dr C. Hunter Stewart. From the Public Health Laboratory, University of Edinburgh. *T.* xxxvii. 743.
3. On the Madder-staining of Dentine. By W. G. Aitchison Robertson, M.D., D.Sc. From the Physiological Laboratory of the University of Edinburgh. Communicated by Professor Rutherford. *P.* xx. 14.
4. On some recent Innovations in Vector Theory. By Professor C. G. Knott. *P.* xix. 212.

*Tuesday, 10th January* 1893.

Professor Sir W. Turner, F.R.S., Vice-President, in the Chair.

The following Communications were read :—

1. On some Modifications of the Water-bottle and Thermometer for Deep Sea Research. By JOHN Y. BUCHANAN, Esq., M.A., F.R.S. (The instruments were exhibited.) *P.* xix. 238.

2. On Electrolytic Synthesis of Dibasic Acids. Part VI.—On the Electrolysis of Ethyl Potassium Dialkylmalonates, and on Secondary Reactions occurring in the Electrolysis of Ethyl Potassium Salts of Dibasic Acids. By Professor CRUM BROWN, F.R.S., and Dr JAMES WALKER. *P.* xix. 243 (*Abstract*). *T.* xxxvii. 361.

3. On the Comparative Histology and Experimental Physiology of the Spleen. By ARTHUR J. WHITING, M.D. From the Physiological Laboratory of the University of Edinburgh. Communicated by Professor RUTHERFORD. *P.* xix. 21 (*Abstract*).

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*Monday, 16th January* 1893.

Professor Copeland, Vice-President, in the Chair.

The following Communications were read :—

1. Obituary Notice of the late Dr Keiller. By Dr T. A. G. BALFOUR. *P.* xix. xxxvi.

2. Obituary Notice of the late Sir George Macleod. By the Rev. W. H. MACLEOD, B.A. (Cantab.), B.D. *P.* xix. xl.

3. On the Present State of Knowledge and Opinion in regard to Colour Blindness. By WILLIAM POLE, F.R.S.L. and F.R.S.E. *T.* xxxvii. 441. *P.* xx. 103.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

DONALD BEITH, Esq., W.S.
ALEXANDER LOW BRUCE, Esq.
ALEXANDER EDINGTON, M.B., C.M.

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*Monday, 30th January* 1893.

Professor Sir Douglas Maclagan, President, in the Chair.

The following Communications were read :—

1. Obituary Notice of the late Sir George Airy. By Professor COPELAND. *P.* xix. iii.

2. Obituary Notice of the late Sheriff Forbes Irvine. By Sheriff Æneas Mackay. *P.* xix. xxiii.

3. A New Solution of Sylvester's Problem of the Three Ternary Equations. By the Hon. Lord M'Laren. *P.* xix. 264.

4. On some Observations made, without a Dust-Counter, on the Hazing Effects of Atmospheric Dust. By John Aitken, F.R.S. *P.* xx. 76.

5. Induction through Air and Water, at great distances, without the use of Parallel Wires. By C. A. Stevenson, M.Inst.C.E. *P.* xx. 25.

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*Monday, 6th February* 1893.

Sir Arthur Mitchell, K.C.B., Vice-President, in the Chair.

The following Communications were read :—

1. On the Particles in Fogs and Clouds. By John Aitken, F.R.S. *T.* xxxvii. 413. *P.* xix. 260 (*Abstract*).

2. On a New Apparatus for Counting Bacterial Colonies in Roll Cultures. By J. Buchanan Young, M.B., B.Sc. From the Public Health Laboratory of the University of Edinburgh. Communicated by Sir Douglas Maclagan. *P.* xx. 28.

3. Preliminary Note on the Hygrometric State of the Atmosphere at Ben Nevis Observatory. By Mr A. J. Herbertson. Communicated by Dr Buchan. *P.* xx. 177.

4. On Properties of the Parabola. By Professor Anglin. *P.* xx. 35.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

Dr Frederick W. Barry.
Arthur George Perkin, Esq.
R. S. Fancourt Barnes, M.D.
Patrick Hehir, M.D.

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*Monday, 20th February* 1893.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read :—

1. On the Structure of the *Eurypteridæ*. By Malcolm Laurie, B.A. Communicated by Dr Ramsay Traquair. *T.* xxxvii. 509.

2. On the Early History of some Scottish Mammals and Birds. By the Rev. Professor Duns. *P.* xx. 50.

3. On the Digestion of Sugar in Health. By W. G. Aitchison Robertson, M.D., D.Sc. Communicated by Professor Rutherford. From the Physiological Laboratory of the University of Edinburgh. *P.* xx. 30 (*Abstract*).

*Monday, 6th March* 1893.

Professor Geikie, Vice-President, in the Chair.

The following Communications were read :—

1. Obituary Notice of Professor J. Couch Adams. By the ASTRONOMER-ROYAL FOR SCOTLAND. *P.* xx.

2. A new Algebra, by means of which Permutations may be transformed in a variety of ways, and their Properties investigated. By T. B. SPRAGUE, M.A. *T.* xxxvii. 399.

3. On the Compressibility of Liquids, in connection with their Molecular Pressure. By Professor TAIT. *P.* xx. 63.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

C. G. H. KINNEAR, F.R.I.B.A.
ROBERT HOWDEN, M.B., C.M.
GEORGE SANDISON BROCK, M.D., C.M.
W. L. CALDERWOOD, Esq.

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*Monday, 20th March* 1893.

Sir Arthur Mitchell, K.C.B., Vice-President, in the Chair.

The following Communications were read :—

1. Preliminary Note on Observations of the Minor Planet Victoria in 1889. By Dr D. GILL, H.M. Astronomer at the Cape of Good Hope. *P.* xx. 47.

2. On a remarkable Glacier-Lake, formed by a branch of the Hardanger-Jökul, near Eidfiörd, Norway. By Dr ROBERT MUNRO. *P.* xx. 53.

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*Monday, 3rd April* 1893.

Sir Douglas Maclagan, M.D., President, in the Chair.

Specimens and Preparations of the New Opium Alkaloid, Xanthaline, discovered by Messrs T. and H. SMITH & Co., were exhibited and remarked on by Mr HILL.

The following Communication was read :—

Alexander Rangabes; Poet, Statesman, and Archæologist. By Emeritus Professor BLACKIE.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

Professor Ludwig Becker, Ph.D.
Joseph Tillie, M.D.

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*Monday, 1st May* 1893.

Sir Douglas Maclagan, M.D., President, in the Chair.

The following Communications were read :—

1. Breath-Figures. By John Aitken, Esq. *P.* xx. 94.
2. On the General Eliminant of Three Equations of Different Degrees. By the Hon. Lord M'Laren.
3. On certain Concretions from the Lower Coal Measures, and the Fossil Plants which they contain. By H. B. Stocks, Esq. Communicated by Dr John Murray. *P.* xx. 69.

The President announced that the Geographical Society of Berlin had awarded the Humboldt Medal to the Challenger Expedition, and had resolved to place the Medal in the hands of Dr John Murray as the personal Representative of the Achievements of the great Enterprise.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

George A. Berry, M.D., F.R.C.S.
J. Macvicar Anderson, Esq., Architect.
Walter E. Archer, Esq.

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*Monday, 15th May* 1893.

Sir William Turner, M.B., F.R.S., Vice-President, in the Chair.

The following Communications were read :—

1. On the genus *Helodus*, Agassiz. By Dr Ramsay H. Traquair, F.R.S.
2. Preliminary Account of a Natural History Collection made on a Voyage to the Gulf of St Lawrence and Davis Straits. By Mr Alexander Rodger. Communicated by Professor D'Arcy Thompson. *P.* xx. 154.

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*Monday, 5th June* 1893.

Sir Douglas Maclagan, M.D., President, in the Chair.

The following Communications were read :—

1. Preliminary Note on the Compressibility of Aqueous Solutions, in connection with Molecular Pressure. By Professor Tait. *P.* xx. 141 (*Abstract*).

2. On an observed Relation between the Carbonic Acid and the added Moisture in the Air of Inhabited Rooms. By CHARLES HUNTER STEWART, B.Sc., M.B. From the Public Health Laboratory of the University of Edinburgh.

3. Approximate Determination of the Path of a Rotating Spherical Projectile. By Professor TAIT. *T.* xxxvii. 427.

4. Graphic Process for the Attraction of a Solid of Revolution on a Particle in its Axis. By G. ROMANES, Esq. Communicated by Professor TAIT.

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*Monday, 19th June* 1893.

The Hon. Lord M'Laren, Vice-President, in the Chair.

Dr James E. Talmage, Curator of the Deseret Museum, Salt Lake City, Utah, exhibited Crystals of Selenite of extraordinary size and purity from the County of Wayne in Southern Utah. The occurrence of these Crystals has been described by Dr Talmage in *Science*, vol. xxi. (No. 524), pp. 85–87, New York, Feb. 1893. Dr Talmage presented two very beautiful specimens to the Society, and was thanked for his communication and for the specimens by the Chairman.

The following Communications were read :—

1. Obituary Notice of Professor James Thomson. By Professor TAIT.

2. Obituary Notice of Professor Dittmar. By Professor CRUM BROWN, F.R.S. *P.* xx. iii.

3. On the Physical Geography of the Clyde Sea Area. Part III. Temperature. By Dr H. R. MILL. *T.* xxxviii. 1.

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*Monday, 3rd July* 1893.

Professor James Geikie, F.R.S., Vice-President, in the Chair.

An invitation, to Fellows, to attend the Adelaide Meeting of the *Australasian Association for the Advancement of Science*, was read.

The following Communications were read :—

1. On the Chemical Composition of Sea Water. By JOHN GIBSON, Ph.D., Professor of Chemistry in the Heriot-Watt College. *P.* xx. 315.

2. On the Hourly Variation of the Rainfall at Ben Nevis Observatory. By ALEXANDER BUCHAN, LL.D.

3. On the Path of a Rotating Spherical Projectile, II. By Professor TAIT. *T.* xxxvii. 427.

4. An Experimental Study of Intra-ocular Therapeutics. By Dr CHASSEAUD. Communicated by Dr NOËL PATON.

The following Candidate for Fellowship was balloted for, and declared duly elected a Fellow of the Society :—

The Rev. JOHN M'MURTRIE, M.A., D.D.

*Monday, 17th July* 1893.

Sir Douglas Maclagan, M.D., President, in the Chair.

The following Communications were read:—

1. Magnetic Induction in Iron and Steel Tubes. By Professor C. G. Knott and A. Shand, Esq.
2. Attraction by Graphic Processes. By G. Romanes, Esq. Communicated by Professor Tait.
3. Observations on the Development of the Human Brain. By Johnson Symington, M.D.
4. On Species of Penguin observed during the Sealing Voyage of the S.S. "Active" in the Neighbourhood of Erebus and Terror Gulf. By C. M. Donald, M.D. Communicated by Professor D'Arcy W. Thompson. *P.* xx. 170.
5. The Diurnal Fluctuations of the Barometer on Ben Nevis during Clear and Foggy Weather respectively. By Alexander Buchan, LL.D.
6. Elimination of Powers of Sines and Cosines between two Equations. By the Hon. Lord M'Laren. *P.* xx. 145.
7. The Chairman reviewed the work of the Session.

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## Meetings of the Royal Society—Session 1893–94.

### GENERAL STATUTORY MEETING.

*Monday, 27th November* 1893.

The following Council were elected:—

*President.*

Sir DOUGLAS MACLAGAN, M.D., F.R.C.P.E.

*Vice-Presidents.*

Sir Arthur Mitchell, K.C.B., LL.D.
Sir William Turner, M.B., F.R.S.
Professor Copeland, Astronomer-Royal for Scotland.
Professor James Geikie, LL.D. F.R.S.
The Hon. Lord M'Laren, LL.D.
The Rev. Professor Flint, D.D.

*General Secretary*—Professor P. G. Tait.

*Secretaries to Ordinary Meetings.*

Professor Crum Brown, F.R.S.
John Murray, Esq., LL.D.

*Treasurer*—Adam Gillies Smith, Esq., C.A.

*Curator of Library and Museum*—Alexander Buchan, Esq., M.A., LL.D.

*Ordinary Members of Council.*

Rev. J. SUTHERLAND BLACK, M.A.
ROBERT KIDSTON, Esq., F.G.S.
Professor JOHN GIBSON, Ph.D.
Professor JAMES BLYTH, M.A.
Professor D'ARCY THOMPSON.
Professor J. SHIELD NICHOLSON.
Professor CHRYSTAL, LL.D.
Dr J. BATTY TUKE, F.R.C.P.E.
Dr ALEXANDER BRUCE, M.A., F.R.C.P.E.
Professor FREDERICK O. BOWER, M.A., F.R.S.
Professor J. G. M'KENDRICK, LL.D., F.R.S.
A. BEATSON BELL, Esq., Advocate.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council:—

HIS GRACE THE DUKE OF ARGYLL, K.G., K.T., LL.D., D.C.L.
THE RIGHT HON. LORD MONCREIFF of Tulliebole, LL.D.
THE RIGHT HON. LORD KELVIN, LL.D., D.C.L., P.R.S., Foreign Associate of the Institute of France.

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*Monday, 4th December* 1893.

Professor Copeland, Astronomer-Royal for Scotland, Vice-President, in the Chair.

The following Communication was read:—

On Bistratification in the Growth of Languages, with special reference to Greek. By Emeritus Professor BLACKIE. *T.* xxxvii. 615.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

The Rev. THOMAS HARDIE TURNBULL.
Mr JAMES MACDONALD.

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*Monday, 18th December* 1893.

The Rev. Professor Duns in the Chair.

The following Communications were read:—

1. Note on the Focus of Concavo-convex Lenses, the Surfaces of which are of Equal Curvature. By Dr GEORGE BERRY, F.R.C.S. *P.* xx. 192.
2. On Torsional Oscillations of Wires. By Dr WILLIAM PEDDIE.
3. On certain Electrical Properties of Iron Occluding Gases. By Mr S. KIMURA, Graduate in Physical Science of the Imperial University of Tokyo, Japan. Communicated by Professor C. G. KNOTT. *P.* xx. 203.
4. The Ether—an Idea of Sir John Herschel Modernised. By Mr S. TOLVER PRESTON. Communicated by Professor C. G. KNOTT.

*Monday, 15th January* 1894.

Sir Douglas Maclagan, M.D., President, in the Chair.

The following Communications were read :—

1. Obituary Notices of the late :—
   (*a*) Dr William F. Skene, Historiographer-Royal for Scotland. By Professor MACKINNON. *P.* xx. xxxiii.
   (*b*) Dr William Burns Thomson, F.R.C.P.E. By Dr JAMES L. MAXWELL.
2. On Two Stereo-isomeric Hydrazones of Benzoin. By Professor ALEXANDER SMITH, B.Sc., Wabash College, Indiana, U.S.A. *P.* xx. 201.
3. On the Compression of Fluids. By Professor TAIT. *P.* xx. 245 (*Abstract*).

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

FRANCIS JOHN ALLAN, M.D., C.M.
J. ANGUS CAMERON, M.D.
Mr HERBERT BOLTON.
Principal JOHN COOK, M.A.
CHARLES HENRY GATTY, M.A., LL.D., F.L.S.

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*Monday, 29th January* 1894.

Professor Copeland, Astronomer-Royal for Scotland, in the Chair.

The following Communications were read :—

1. On the Rate of Fermentation of Sugars. By W. G. AITCHISON ROBERTSON, M.D., D.Sc. Communicated by Professor RUTHERFORD. *P.* xx. 164 (*Abstract*).
2. The Indian Currency Experiment, with special reference to the Foreign Trade of India. By Professor J. S. NICHOLSON.

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*Monday, 5th February* 1894.

Dr Copeland, Astronomer-Royal for Scotland, Vice-President, in the Chair.

At the request of the Council, Dr JOHN MURRAY gave an Address "On the Floor of the Ocean at Great Depths."

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

Lieut.-Col. FREDERICK BAILEY.
Professor JOHN STRUTHERS, M.D., LL.D.
ALFRED HILL, M.D., M.R.C.S.
JAMES BURGESS, C.I.E., LL.D.
Mr ARCHIBALD DENNY.

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*Monday, 19th February* 1894.

Sir Douglas Maclagan, M.D., President, in the Chair.

The following Communications were read :—

1. Obituary Notice of Professor Alphonse Louis Pierre Pyramus de Candolle. By Professor FREDERICK O. BOWER, F.R.S. *P.* xx.

2. On the Number of Dust Particles in the Atmosphere of Certain Places in Great Britain and on the Continent :—with Remarks on the Relation between the Amount of Dust and Meteorological Phenomena. Part III. By JOHN AITKEN. *T.* xxxvii. 621.

3. Suggestion as to the possible Nature of Electrification. By GEORGE ROMANES, Esq. Communicated by Dr C. G. KNOTT.

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*Monday, 5th March* 1894.

Sir William Turner, F.R.S., Vice-President, in the Chair.

The following Communications were read :—

1. Obituary Notice of Professor William Morse Graily Hewitt, M.D. By Professor A. R. SIMPSON, M.D. *P.* xx. v.

2. On the Division of a Parallelepiped into Tetrahedra. Part I.—The Cube. By Professor CRUM BROWN, F.R.S. *T.* xxxvii. 711.

3. On the Second and Fourth Digits in the Horse,—their Development and Subsequent Degeneration. By Professor J. C. EWART, F.R.S. *P.* xx. 185.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

WALTER JOHN MABBOTT, M.A.
J. M. M. MUNRO, M.I.E.E.
EDWARD WHYMPER, Esq.

*Monday, 19th March* 1894.

Professor Geikie, F.R.S., Vice-President, in the Chair.

The following Communications were read :—

1. Obituary Notice of the Rev. Thomas Brown. By Professor Duns, D.D. *P.* xx. x.

2. On the Division of a Parallelepiped into Tetrahedra. Part II. By Professor Crum Brown, F.R.S. *T.* xxxvii. 711.

3. The Reproduction of the Edible Crab (*Cancer pagurus*). By Gregg Wilson, M.A., B.Sc., Natural History Department, University of Edinburgh. Communicated by Professor Ewart, F.R.S. *P.* xx. 309.

4. Telegraphic Communication by Induction by means of Coils. By C. A. Stevenson, B.Sc., M.Inst.C.E. *P.* xx. 196.

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*Monday, 2nd April* 1894.

Sir Douglas Maclagan, M.D., President, in the Chair.

The Chairman announced that the Council had made the following awards :—

1. The Gunning Victoria Jubilee Prize for 1891–4 to Alexander Buchan, Esq., LL.D.

2. The Keith Prize for 1891–3 to Professor T. R. Fraser, F.R.S.

3. The Makdougall-Brisbane Prize for 1890–92 to H. R. Mill, Esq., D.Sc.

4. The Neill Prize for the period 1889–92 to John Horne, Esq., F.G.S.

At the request of the Council, an Address on "The Climatic Conditions of the Glacial Period" was given by Professor Geikie, F.R.S., Vice-President R.S.E.

A Note on the Antecedents of Clerk-Maxwell's Electrodynamical Equations, by Professor Tait, was laid on the table. *P.* xx. 213.

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*Monday, 7th May* 1894.

Sir William Turner, F.R.S., Vice-President, in the Chair.

At the request of the Council, Dr Robert Munro gave an Address "On the Rise and Progress of Anthropology." *P.* xx. 215.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

John Shand, M.D., F.R.C.P.E.
Malcolm Laurie, B.Sc., B.A., F.L.S.
John Jackson.

*Monday, 21st May* 1894.

Sir Douglas Maclagan, M.D., President, in the Chair.

## PRIZES.

The Gunning Victoria Jubilee Prize for 1890–93 was presented to ALEXANDER BUCHAN, Esq., LL.D., for his varied, extensive, and extremely important contributions to Meteorology, many of which have appeared in the Society's Publications.

The PRESIDENT, on presenting the Prize, said :—

During the past twenty or thirty years, Dr Alexander Buchan has carried on a large number of scientific investigations. Some of these deal with general questions in meteorology, and others with the meteorology of the British Islands—many of the latter treating especially of the meteorology of Scotland.

The results of these researches have been made known in a long series of papers to learned societies.

Among the more important of these may be mentioned :—

"On the Mean Atmospheric Pressure and Prevailing Winds of the Globe."

"The Climatology of the British Islands—Temperature, Pressure, Winds, Rain, and Thunderstorms."

"The Weather and Health of London and other Places" (conjointly with Sir Arthur Mitchell).

"The Storms of Europe."

"The Rainfall of Scotland over Twenty Years."

"Prevailing Winds of Scotland."

"Extreme Temperatures and Rainfall of Scotland."

"Interruptions in the Regular Rise and Fall of Temperatures in the course of the Year."

"On the Meteorology of Ben Nevis: especially on the Change of Pressure and Temperature with Height, and the Influence of Strong Winds on the Barometer."

"On Atmospheric Circulation": one of the "Challenger" Reports, giving, with full and elaborate tables and maps, the mean monthly and annual temperature and pressure of the globe, together with the prevailing winds and diurnal fluctuation of the barometer. In this Report the meteorology of the ocean is for the first time seriously investigated.

Dr Buchan's researches and papers are among the most important contributions to meteorology in recent years, and have placed him in the foremost rank among living meteorologists.

Some of these papers have appeared in the publications of the Society, and most of them have, in the first instance, been communicated to its meetings.

For the above reasons, the Council of the Society have awarded to Dr Buchan *The Triennial Gunning Victoria Jubilee Prize.*

The Keith Prize for 1891–3 was presented to Professor T. R. Fraser, F.R.S., for his papers on *Strophanthus hispidus*, Strophanthin, and Strophanthidin, read to the Society in February and June 1889 and in December 1891, and printed in Vols. XXXV., XXXVI., and XXXVII. of the Society's *Transactions.*

The President, on presenting the Prize, said:—

The Prize Committee recommended that the Keith Prize be awarded to Professor Thomas R. Fraser, F.R.S., for his papers on *Strophanthus hispidus*, read before the Society in February and June 1889, and in December 1891, in which he has given a complete history of this plant, embracing—1st, its botany; 2nd, its chemistry, including the discovery of its active principle, strophanthin; 3rd, its pharmacology, showing its powerful physiological action on the heart; and 4th, its uses in practical medicine as a remedy in heart affections.

The Makdougall-Brisbane Prize for 1890–92 was presented to H. R. Mill, Esq., D.Sc., for his papers on the Physical conditions of the Clyde Sea Area, Part I. being already published in Vol. XXXVI. of the Society's *Transactions.*

The President, on presenting the Prize, said:—

The Prize has been awarded to Dr Mill for his papers on the Physical conditions of the Clyde Sea Area.

These give (1) A Summary of the Physical Geography of the Region, with special reference to the orographical and bathymetrical configuration, and to rainfall; (2) Observations on the Variations of Salinity in the water of different parts of the area for a considerable period; and (3) a discussion of very numerous temperature observations at all depths, extending over several years. The work, as a whole, elucidates the influence exerted by configuration on seasonal heat-changes in sea-water, the nominal sequence of these changes, and the relations between the temperature of water and air.

The Neill Prize for the period 1889–92 was presented to JOHN HORNE, Esq., F.G.S., for his investigations into the Geological Structure and Petrology of the North-West Highlands.

The PRESIDENT, on presenting the Prize, said:—

Mr Horne is one of our most distinguished field geologists, and has borne a leading part in working out the extremely complicated structure of the North-West Highlands—the result of his and his colleague's labours having greatly increased our knowledge of "mountain-building." During the past five years he has published the results of these and other correlated investigations in the *Journal of the London Geological Society*, the *Transactions of the Royal Society of Edinburgh*, and other scientific journals, &c. In conjunction with Mr B. N. Peach he has established the existence in the North-West Highlands of the Olenellus Zone of the Cambrian System—a very important addition to our knowledge of the stratigraphical geology of our islands.

Mr Horne is also distinguished for his researches in glacial geology, and within the present and immediately preceding years (1892, 1893, 1894) has made interesting and valuable communications upon the subject. In 1892 he was appointed chairman of a committee of the Geological Section of the British Association to investigate certain fossiliferous glacial deposits, with a view to ascertaining their exact geographical horizon. The report of this committee, presented at last meeting of the Association, was drafted by Mr Horne, and is a distinct addition to our knowledge.

The following Communications were read:—

1. On some Problems of Evolution. By Professor D'ARCY W. THOMPSON.

2. On a new form of Water-Collecting Bottle. By Mr H. N. DICKSON. *P.* xx. 252.

3. On the Determination of Sea Water Densities by Hydrometers and Sprengel Tubes. By Mr W. S. ANDERSON. Communicated by Dr JOHN MURRAY.

4. On the Origin and Distribution of Manganese Oxides and Manganese Nodules on the Floor of the Ocean. By Dr JOHN MURRAY and Mr ROBERT IRVINE. *T.* xxxvii. 721.

5. On the Constitution of the Earth's Crust on the Continents and beneath the Oceans. By Dr JOHN MURRAY.

*Monday, 28th May* 1894.

Prof. James Geikie, Vice-President, in the Chair.

The following Communications were read:—

1. The Chemical and Bacteriological Examination of Soil. With special reference to the Soil of Grave-Yards. By James Buchanan Young, M.B., D.Sc. From the Public Health Laboratory, University of Edinburgh. *T.* xxvii. 759.

2. The Pallial Complex of *Dolabella* (sp.?). By J. D. F. Gilchrist, M.A., B.Sc., Ph.D. Communicated by Professor Ewart, F.R.S. From the Natural History Laboratory of the University of Edinburgh. *P.* xx. 264.

3. Hydrolysis in some Aqueous Salt-Solutions. By James Walker, D.Sc., Ph.D. *P.* xx. 255.

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*Monday, 4th June* 1894.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read:—

1. On the Application of Van der Waals' Equation to the Compression of Ordinary Liquids. By Professor Tait. *P.* xx. 285.

2. Note on some Fossils from Seymour Island in the Antarctic Regions, obtained by Dr Donald. By Messrs G. Sharman and E. T. Newton. Communicated by Professor Geikie. *T.* xxxvii. 707.

3. On Certain Difficulties in the Study of Classical Zoölogy. By Professor D'Arcy Thompson.

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*Monday, 18th June* 1894.

Sir William Turner, F.R.S., Vice-President, in the Chair.

The following Communications were read:—

1. On the Path of the Meteor of May 18th, 1894. By The Astronomer-Royal for Scotland.

2. On the Elastic Equations of the Ether in Aeolotropic Dielectrics. By Professor Tait.

3. A Comparison of the Extra-tropical Marine Fauna of the Northern and Southern Hemispheres. By Dr John Murray.

4. Further Illustrations of the Range of Application of Van der Waals' Equation. By Professor Tait.

5. Magnetic Induction in Nickel Tubes. By Dr C. G. Knott and A. Shand, Esq. *P.* xx. 290.

*Monday, 2nd July* 1894.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read :—

1. Co-ordinates versus Quaternions. By Professor CAYLEY. *P.* xx. 271.

2. On the Intrinsic Nature of the Quaternion Method. By Professor TAIT. *P.* xx. 276.

3. Note on the Volume Changes which accompany Magnetization in Nickel Tubes. By Dr C. G. KNOTT and A. SHAND, Esq. *P.* xx. 295.

4. On Histological Changes produced in Nerve Cells by their Functional Activity. By GUSTAV MANN, M.B. Communicated by Professor RUTHERFORD.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

ROBERT MACKENZIE, M.D.
WILLIAM SHIELD, M.Inst.C.E.
PHILIP R. D. MACLAGAN.

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*Tuesday, 10th July* 1894.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read :—

1. On the Measurement of the Simple Reaction Time of Hearing, Sight, and Touch. (With Experimental Illustrations.) By Professor RUTHERFORD, F.R.S. *P.* xx. 328.

2. The Estimated Total Amounts of the Principal Substances in Solution in the Ocean, and the Source of these Substances. By Dr JOHN MURRAY.

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*Monday, 16th July* 1894.

Professor Chrystal, Vice-President, in the Chair.

The following Communications were read :—

1. On the Circulation of Water in the Faero-Shetland Channel and the North Sea. By Mr H. N. DICKSON.

2. Note on the Compressibility of the Water of the Great Salt Lake, Utah. By Professor TAIT.

3. On the Solution of Systems of Equations by means of Determinants. By the Hon. Lord M'LAREN.

4. Preliminary Note on the Use of Phosgene in preparing Compound Ethers. By Professor CRUM BROWN.

5. On the Fossil Fishes of Forfarshire. By Dr R. H. TRAQUAIR, F.R.S.

6. The CHAIRMAN reviewed the work of the Session.

## Meetings of the Royal Society—Session 1894–95.

### GENERAL STATUTORY MEETING.

*Monday, 26th November* 1894.

The following Council were elected :—

*President.*

Sir DOUGLAS MACLAGAN, M.D., F.R.C.P.E., LL.D.

*Vice-Presidents.*

Professor Sir William Turner, M.B.
Professor Ralph Copeland, Ph.D., Astronomer-Royal for Scotland.
Professor James Geikie, LL.D., F.R.S.
The Hon. Lord M'Laren, LL.D., F.R.A.S.
The Rev. Professor Flint, D.D.
Professor John G. M'Kendrick, M.D., LL.D., F.R.S.

*General Secretary*—Professor P. Guthrie Tait, M.A., D.Sc.

*Secretaries to Ordinary Meetings.*

Professor Crum Brown, M.D., F.R.S.
John Murray, Esq.,LL.D., Ph.D.

*Treasurer*—Philip R. D. Maclagan, Esq., F.F.A.

*Curator of Library and Museum*—Alexander Buchan, Esq., M.A., LL.D.

*Councillors.*

Professor D'Arcy W. Thompson, B.A.
Professor J. Shield Nicholson, M.A., D.Sc.
Professor George Chrystal, M.A., LL.D.
Dr J. Batty Tuke, F.R.C.P.E.
Dr Alexander Bruce, M.A., F.R.C.P.E.
Professor Frederick O. Bower, M.A., F.R.S.
A. Beatson Bell, Esq., Advocate.
Sir Arthur Mitchell, K.C.B., LL.D.
Professor Thomas R. Fraser, M.D., F.R.S.
Dr Robert Munro, M.A.
Dr D. Noël Paton, B.Sc., F.R.C.P.E.
Cargill G. Knott, Esq., D.Sc.

By a Resolution of the Society (19th January 1880), the following Hon. Vice-Presidents, having filled the office of President, are also Members of the Council :—

His Grace the DUKE of ARGYLL, K.G., K.T., LL.D., D.C.L.
The Right Hon. LORD MONCREIFF of Tulliebole, LL.D.
The Right Hon. LORD KELVIN, LL.D., D.C.L., P.R.S., Foreign Associate of the Institute of France.

*Tuesday, 27th November* 1894.

Professor Copeland, Astronomer-Royal for Scotland, Vice-President, in the Chair.

Professor M'KENDRICK read a Paper on "Observations with the Phonograph, with Experimental Illustrations."

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*Monday, 3rd December* 1894.

Professor Geikie, F.R.S., Vice-President, in the Chair.

The following Communications were read:—

1. Notes on a Peculiarity in the Form of the Mammalian Tooth. By JOHN SMITH, M.D. *P.* xx. 336.
2. The Development of the Müllerian Duct of Amphibians. By GREGG WILSON, M.A., B.Sc. Communicated by Professor EWART.
3. On a Theorem relating to the Difference between any two terms of the Adjugate Determinant. By THOMAS MUIR, LL.D. *P.* xx. 323.
4. A new Method for Correcting Courses:—by an Instrument for the purpose. By Dr GEORGE HAY, Pittsburg, Pa. Communicated by Dr C. G. KNOTT.
5. Note on the Constitution of Volatile Liquids. By Professor TAIT.
6. The Isothermals of Ethylene. By the Same.

Mr R. G. ALFORD and Mr J. E. TALMAGE were balloted for, and declared duly elected Fellows of the Society.

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*Monday, 17th December* 1894.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read:—

1. Obituary Notice of Donald Beith, Esq., W.S. By PATRICK MURRAY, Esq., W.S. *P.* xx. 7.
2. Notes on Germination in Ponds and Rivers. By H. B. GUPPY, Esq., M.B.
3. Experiments on the Hall Effect, and on some related actions in Bismuth. By J. C. BEATTIE, Esq. Communicated by Professor TAIT. *T.* xxxviii.
4. Attraction by Graphic Processes.—Deductions. By GEORGE ROMANES, Esq. Communicated by Professor TAIT.

*Monday, 7th January* 1895.

The Rev. Professor Flint, D.D., Vice-President, in the Chair.

The following Communications were read :—

1. On a Case of Yellow-Blue Colour Blindness, and its Bearings on the Theory of Dichromasy. By W. PEDDIE, Esq., D.Sc. *Tr.* xxxviii.
2. On Metabolism in Thyroid Feeding. By JOHN DOUGLAS, M.B. *P.* xx. 330. From the Laboratory of the Royal College of Physicians.
3. Anatomy of Vermiform Process and Cæcum. By RICHARD BERRY, M.B. From the Laboratory of the Royal College of Physicians. Communicated by Dr NOËL PATON.
4. On the Ultimate State of a System of Colliding Particles, and the Rate of Approach to it. By Professor TAIT.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

ALBERT H. TURTON, F.C.S.
CHARLES BRIGHT, Assoc. M. Inst. C.E., M.I.E.E.

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*Monday, 21st January* 1895.

Professor Sir William Turner, F.R.S., Vice-President, in the Chair.

The following Communications were read :—

Obituary Notices of 1. General Robert Maclagan, R.E., LL.D., F.R.A.S. By Major-General R. MURDOCH SMITH, K.C.M.G., R.E. *P.* xx.
2. Alexander Leslie, M. Inst. C.E. By JAMES BRAND, Esq. *P.* xx.
3. William Durham. By Professor C. G. KNOTT, D.Sc. *P.* xx.
4. On the Absorption of Carbohydrates from the Intestine. By G. LOVELL GULLAND, M.D., and D. NOËL PATON, M.D., F.R.C.P.E. *P.* xx. 347.
5. On the Torsion of the Molluscan Body. By J. D. F. GILCHRIST, B.Sc., Ph.D. Communicated by Professor EWART, F.R.S. *P.* xx. 357.
6. On a Curious Property of Determinants. By Professor TAIT.

Mr PHILIP R. D. MACLAGAN was admitted a Fellow of the Society.

*Monday, 4th February* 1895.

Professor J. G. M'Kendrick, F.R.C.P.E., LL.D., F.R.S., Vice-President, in the Chair.

The following Communications were read:—

1. Obituary Notice of Dr Sanderson. By Dr BUCHAN. *P.* xx.
2. Note on Normal Nystagmus. By Professor CRUM BROWN, F.R.S. *P.* xx. 352.
3. On M. Dubois' account of Pithecanthropoid Remains recently found in Java. By Sir W. TURNER, F.R.S. *P.* xx. 422.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

The Most Hon. The MARQUIS OF LOTHIAN, K.T.
JOHN MACINTYRE, M.D.
Surgeon-Major HENRY HALCRO JOHNSTON, D.Sc., M.D.

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*Monday, 18th February* 1895.

The Hon. Lord M'Laren, LL.D., Vice-President, in the Chair.

The following Communications were read:—

1. A Theorem regarding the Equivalence of Systems of Ordinary Linear Differential Equations with Constant Coefficients, and its Application to the Theory of such Systems. By Professor CHRYSTAL, LL.D. *T.* xxxviii. 163.
2. Note on Volume-Changes in Iron and Nickel Tubes when Magnetized. By C. G. KNOTT, D.Sc., and A. SHAND. *P.* xx. 334.
3. On a Case of Yellow-Blue Blindness. Comparison with the Case described by v. Vintschgau and Hering. By W. PEDDIE, D.Sc. *T.* xxxviii.

Surgeon-Major H. H. JOHNSTON was admitted a Fellow of the Society.

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*Monday, 4th March* 1895.

Sir Douglas Maclagan, M.D., President, in the Chair.

At the request of the Council, Dr MUNRO gave an address—"A Sketch of Lake-Dwelling Research." *P.* xx. 385.

The following Candidate for Fellowship was balloted for, and declared duly elected a Fellow of the Society :—

Thomas Oliver, M.D., F.R.C.P., Professor of Physiology in the University of Durham.

Mr R. G. Alford was admitted a Fellow of the Society.

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*Monday, 18th March* 1895.

The Rev. Professor Flint, D.D., Vice-President, in the Chair.

The following Communications were read :—

1. Note on the action of Sodium Mercaptide on Dibromomalonic Ether. By Professor Crum Brown, F.R.S., and Robert Fairbairn, B.Sc. *P.* xx. 336.
2. The Dorsal Branches of Cranial and Spinal Nerves in Elasmobranchs. By J. C. Ewart, F.R.S., and F. J. Cole. *P.* xx. 475.
3. On Phosphorescent Sandstones. By Dr R. H. Traquair, F.R.S.
4. Note on the Electro-magnetic Wave Surface. By Professor Tait.

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*Monday, 1st April* 1895.

Sir Douglas Maclagan, M.D., President, in the Chair.

A Paper on "The Glaciation of Two Glens," by His Grace The Duke of Argyll, was read at the request of the Council. *T.* xxxviii. 193.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society :—

D. Deuchar, F.I.A., F.F.A.
James Napier, M.A.
George Sandeman, M.A.

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*Wednesday, 17th April* 1895.

Professor Sir Douglas Maclagan, M.D., President, in the Chair.

At the request of the Council, Professor W. M. Flinders Petrie, D.C.L., delivered a Lecture "On a New Race in Egypt." (With Lantern Illustrations.)

Mr David Prain and Mr D. Deuchar were admitted Fellows of the Society.

*Monday, 6th May* 1895.

Professor Sir William Turner, Vice-President, in the Chair.

The following Communications were read:—

1. On Drops. By J. BALLANTYNE HANNAY. *P.* xx. 437.
2. Further Note on a Problem of Sylvester's in Elimination. By THOMAS MUIR, LL.D. *P.* xx. 371.
3. On the Results of some Experiments with Potash Manures. By R. PATRICK WRIGHT, F.H.A.S., West of Scotland Technical College. Communicated by Dr A. P. AITKEN.
4. On the Conditions for a Kink in the Path of a Projectile. By Professor TAIT.
5. Systems of Plane Curves whose Orthogonals form a similar System. By Professor TAIT. *P.* xx. 497.

Professor OLIVER was admitted a Fellow of the Society.

The following Names proposed by the Council were balloted for, and these were elected:—

*As British Honorary Fellows.*

Sir J. WILLIAM DAWSON, C.M.G., LL.D., F.R.S., late Principal of M'Gill University, Montreal.

ALBERT C. L. G. GÜNTHER, Ph.D., F.R.S., Zoological Department in the British Museum, London.

JOHN RUSSELL HIND, LL.D., F.R.S., Corresponding Member of the Institute of France.

Sir CHARLES TODD, K.C.M.G., F.R.S., Government Astronomer, Adelaide, South Australia.

*As Foreign Honorary Fellows.*

LUDWIG BOLTZMANN, Professor of Physics in the University of Vienna.

GABRIEL AUGUSTE DAUBRÉE, of the Institute of France.

ÉLEUTHÈRE-ÉLIE-NICOLAS MASCART, Professor of Physics and Meteorology, Paris.

CARL MENGER, Professor of Political Economy in the University of Vienna.

MAX VON PETTENKOFER, Professor of Hygiene in the University of Munich.

JULES HENRI POINCARÉ, of the Institute of France.

*Monday, 20th May* 1895.

Professor Copeland, Vice-President, in the Chair.

The following Communications were read:—

1. Hygrometric Researches at Ben Nevis, Fort William, and Montpellier. By Mr A. J. HERBERTSON. Communicated by Professor TAIT. *T.* xxxviii.

2. On the Shapes of Leaves. By Professor D'ARCY THOMPSON.

Dr ROBERT MACKENZIE was admitted a Fellow of the Society.

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*Monday, 3rd June* 1895.

The Hon. Lord M'Laren, Vice-President, in the Chair.

The following Communications were read:—

1. On a Case of Dichromasy: Disc, Spectrum, and Contrast Tests. By Dr W. PEDDIE. *T.* xxxviii.

2. On the Rendering of Animals Immune against the Venom of the Cobra and other Serpents; and on the Antidotal Properties of the Blood-Serum of the Immunized Animals. By Professor THOMAS R. FRASER, F.R.S. *P.* xx. 448.

The following Candidates for Fellowship were balloted for, and declared duly elected Fellows of the Society:—

Dr THOMAS SAVAGE, Professor of Gynæcology, Mason College, Birmingham.

DAVID WILSON BARKER, Captain-Superintendent Thames Nautical Training College, H.M.S. "Worcester," Greenhithe, Kent.

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*Monday, 17th June* 1895.

Professor Chrystal in the Chair.

The following Communications were read:—

1. On the Relation between the Variation of Resistance in Bismuth, in a steady magnetic field, and the Rotatory or Transverse Effect. By Mr J. C. BEATTIE. Communicated by Dr C. G. KNOTT. *T.* xxxviii.

2. On Torsional Oscillations of Wires. By Dr W. PEDDIE.

3. On the Marine Fauna of the Great Southern Ocean. By Dr JOHN MURRAY. *T.* xxxviii.

Mr WILLIAM THOMSON was admitted a Fellow of the Society.

*Monday, 1st July* 1895.

Professor M'Kendrick, M.D., F.R.S., Vice-President, in the Chair.

The Chairman announced that the Council had awarded the Makdougall-Brisbane Prize for the Biennial Period, 1892–94, to Professor JAMES WALKER, University College, Dundee, for his work on Physical Chemistry, part of which has been published in the *Proceedings* of the Society, Vol. XX. pp. 255–263. In making this award, the Council took into consideration the work done by Professor Walker along with Professor Crum Brown on the Electrolytic Synthesis of Dibasic Acids, published in the *Transactions* of the Society.

The following Communications were read :—

1. Obituary Notice of the late Dr Hugh Cleghorn. By Professor W. C. M'INTOSH, F.R.S. *P.* xx.
2. On the Granular Leucocytes. By G. LOVELL GULLAND, M.D. Communicated by Dr NOËL PATON.
3. Preliminary Note on the Thermo-electric Properties of hot and cold, chemically similar, Metals. By W. PEDDIE, D.Sc., and A. H. FIRTH, Esq.
4. On the Secretion of Carbonate of Lime by Marine Organisms at different Temperatures. By Dr JOHN MURRAY and ROBERT IRVINE, Esq.

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*Monday, 15th July* 1895.

Sir Douglas Maclagan, M.D., President, in the Chair.

The President presented the Makdougall-Brisbane Prize for the Biennial Period, 1892–94, to Professor JAMES WALKER, University College, Dundee, for his work on Physical Chemistry.

When the Prize was presented, Professor Crum Brown said—

Successful work of the kind done by Professor Walker requires not only great experimental skill and a sound knowledge of Chemistry, but also a comprehensive grasp of the relations of the physical sciences and familiarity with the mathematical methods necessary for intelligently discussing the phenomena. These qualifications Professor Walker possesses, and he has applied them with great success.

As examples of his work I may mention his papers on "The Vapour Pressure of Aqueous Solutions," "The Affinity Constants of Weak Bases and of Organic Acids," "The Hydrolysis of Salts," and "The Boiling Points of Homologous Compounds."

I cannot allow this opportunity to pass without testifying to Professor Walker's great ability as an investigator in pure Chemistry. Besides the investigations which he and I carried on together, and of which the results were published in the Society's *Transactions*, he has, since he left Edinburgh, published valuable papers on Camphoric Acid and its Derivatives, which throw much light on their constitution.

The following Communications were read:—

1. Obituary Notice of the late Thomas Stevenson, M.Inst. C.E. By the late Professor Swan. *P.* xx.

2. Specific Gravities and Oceanic Circulation. By Dr Buchan. *T.* xxxviii.

3. Further Observations on Antivenene, and on the Production of Immunity against Serpents' Venom; with an Account of the Antidotal Properties of the Blood-Serum of Venomous Serpents. By Professor Fraser, M.D., F.R.S. *P.* xx. 465.

4. Determination of the Co-efficient of Resistance of Air to a moving Sphere. By Professor Tait.

5. On the Geometrical Type of the Surfaces of Univalve Shells. By S. Kimura, Esq. Communicated by Dr Knott.

6. Nine-place Tables of $\frac{2}{\sqrt{\pi}}\int_0^t \epsilon^{-t^2} dt$ to $t = 1{\cdot}18$. By Dr James Burgess, C.I.E.

7. On the Fossil Flora of the Yorkshire Coal Field. (First Paper.) By Robert Kidston, Esq. *T.* xxxviii.

8. On the Behaviour of Various Alloys in a Steady Magnetic Field. By Mr J. C. Beattie. *P.* xx.

9. On the Variation of Resistance in a Steady Magnetic Field observed in Nickel, Antimony and Tellurium Plates. By the Same. *P.* xx.

10. Remarks on the Work of the Recent Session. By the President.

# Donations to the Library of the Royal Society from 1893 to 1894.

## I. Transactions and Proceedings of Learned Societies, Academies, &c.

*Adelaide.—Philosophical Society.* Transactions and Proceedings. Vols. XVI.–XVIII. 1892–94. 8vo.

*University.* Calendar for 1894.

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*Amsterdam.—Kon. Akademie van Wetenschappen.* Verhandelingen. Afd. Natuurkunde. 1ste Sectie. Deel I. 2. 1893–94. 2te Sectie. Deel I., II. No. 1, III.—Afd. Letterkunde. Deel I. 1, 2, 3. 1893–94.—Verslagen en Mededeelingen, Natuurkunde. 3e Rks., Dl. IX. 1893.—Letterkunde. 3e Rks., Dl. 9, 10. 1893–94.—Jaarboek, 1892–93.—Poemata Latina.

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Flora Batava. 301–308 Afleveringen. (*From the Dutch Government.*)

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*Baltimore.—Johns Hopkins University.* American Journal of Mathematics. Vols. XV., XVI. 1893–94.—American Chemical Journal. Vols. XV., XVI. 1893–94.—American Journal of Philology. Vols. XIII., XIV., XV. 1. 1893–94.—Studies from the Biological Laboratory of the Johns Hopkins University. Vol. V. 2–4. 1894. 8vo.—University Studies in Historical and Political Science. 11th and 12th Series.—University Circulars. 1893–94.

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*Basel.—Naturforschende Gesellschaft.* Verhandlungen. Bd. X. 1893–94. 8vo.

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*Berlin.—K. Akademie der Wissenschaften.* Abhandlungen, 1892–93.—Sitzungsberichte. 1893–94.

*Physikalische Gesellschaft.* Fortschritte der Physik im Jahre 1887–88. 1[te] Abtheil.—Allgemeine Physik, Akustik. 2[te] Abtheil.—Optik, Wärmelehre, Elektricitätslehre. 3[e] Abtheil.—Physik der Erde. Berlin. 8vo.

*Deutsche Meteorologische Gesellschaft.* Zeitschrift. 1893–94. 8vo.

*Preussisches Meteorologisches Institut.* Ergebnisse der Meteorologischen Beobachtungen im Jahre 1893. 4to.—Ergebnisse der Niederschlags-Beobachtungen im Jahre 1891, 1892. 4to.—Ergebnisse der Beobachtungen an den Stationen II. und III. Ordnung im 1894. 4to.—Berichte, 1891–93.

*Deutsche Geologische Gesellschaft.* Zeitschrift. Bde. I.–XVI., XXIV.–XLVI. 8vo.

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*Bologna.—Accademia d. Scienze dell' Istituto di Bologna.*
Memorie. Ser. V., Tom. II., III. 1891–92.

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# OBITUARY NOTICES.

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## John Couch Adams. By Professor Copeland.

(Read March 6, 1893.)

At Lidcot farm, in the rural parish of Laneast, some seven miles to the west of Launceston, in Cornwall, on June 5th, 1819, was born John Couch Adams, whose name will ever be inseparably associated with the discovery of Neptune. Educated at Devonport, he entered St John's College, Cambridge, in October 1839. He graduated as Senior Wrangler and first Smith's Prizeman early in 1843, and shortly afterwards was elected a Fellow of his college, and became one of its mathematical tutors. As a student, he had read in Airy's *Report on the Progress of Astronomy during the Present Century* about certain unexplained perturbations of Uranus, as shown by Bouvard's tables of that planet, and at once perceived that they probably arose from the action of an unknown member of the solar system. Seeing, however, that no merely superficial research could throw light on the subject, he, for the time, contented himself with jotting down on Saturday, July 3rd, 1841, the following memorandum :—"Formed a design, in the beginning of this week, of investigating, as soon as possible after taking my degree, the irregularities in the motion of Uranus which are yet unaccounted for; in order to find whether they may be attributed to the action of an undiscovered planet beyond it, and, if possible, thence to determine approximately the elements of its orbit, &c., which would probably lead to its discovery."

The investigation must indeed have been taken up immediately after his graduation, for already, in 1843, by combining the modern observations with the residuals of Bouvard's equations, on the assumption of a circular orbit for the unknown planet, a first solution was obtained which showed that an agreement between observation and theory might be brought about. Further, earlier data still wanting were supplied by the Astronomer-Royal, who, in February 1844, sent to Adams all the Greenwich observations of Uranus.

In other quarters the irregular motion of Uranus had attracted attention. Our countryman, Dr Hussey, had proposed an extended search for an outer planet, combined with a partial survey of the heavens. The illustrious Bessel had devoted considerable time to an attempted explanation, at first on the hypothesis of an elective attraction on the part of Saturn. Failing health compelled him to hand the work over to one of his assistants, whose health in turn also gave way before anything was accomplished beyond a reduction of the older observations. The Royal Academy of Sciences of Göttingen, however, proposed the Theory of Uranus as their mathematical prize; and although Adams tells us that his college duties prevented him from attempting the complete examination of the theory, which a competition for the prize would have required, yet this fact, together with the possession of such a valuable series of observations, induced him to undertake a new solution of the problem. With indomitable perseverance the subject was now attacked by the sure method of successive approximations. Not one solution, but several solutions were obtained, differing little from each other. Gradually more and more terms of the perturbational series were taken into account, until at last, in September 1845, he was able to communicate to Professor Challis the definite values he had obtained for the mass, the heliocentric longitude, and the elements of the orbit of the assumed planet. Slightly corrected results were communicated to the Astronomer-Royal a month later. But Adams did not rest content. The excentricity being larger than was probable, the whole investigation was again repeated, using a less mean distance. The final result was communicated to Mr Airy in the beginning of September 1846.

Meanwhile, on November 10th, 1845, the brilliant young French astronomer, Le Verrier, had presented to the French Academy a most thorough investigation of the orbit of Uranus as perturbed by Saturn and Jupiter, taking into account several minute perturbations neglected by Bouvard. This he followed up by a second memoir presented on the 1st of June 1846 (or nine months later than

Adams's decisive communications to Challis and Airy), in which the outstanding perturbations of Uranus were explained by the action of a planet whose position agreed very closely with that indicated by Adams. This close agreement by two investigators, each working in ignorance of what was being done by the other, at once set Professor Challis to work on a search for the planet, but the want of a proper star-map necessitated the survey of a relatively considerable area of the heavens. In the course of this survey the planet was actually seen on August 4th and 12th, 1846, but failing a comparison of the observations, it was not then recognised as the object so eagerly sought for. That no search was made at Greenwich is explained by the simple fact that they had no telescope at all suited to the work.

On the last day of this month of August 1846, Le Verrier submitted to the Academy in Paris a third memoir, in which the mass of the unknown planet was worked out, together with new elements and limiting values for its heliocentric place. Eighteen days afterwards, Le Verrier wrote to Dr Galle, then Encke's assistant at Berlin, asking him to look for the planet in the assigned place, and holding out a hope that it might even be recognised by its disc. The planet was found the very day, September 23rd, on which Le Verrier's letter reached Berlin. Everything favoured the search—Galle was an accomplished observer, the instrument was one of Fraunhofer's masterpieces, and Galle cordially accepted the aid of the young astronomer D'Arrest, then a student at the Berlin Observatory. D'Arrest contributed notably to the immediate finding of the planet, by suggesting the use of Bremiker's section (Hora xxi.) of the Equatorial Star-maps, then in course of publication by the Berlin Academy. This very sheet had just been struck off, but had not yet been distributed, although a copy was lying at the Berlin Observatory. Galle estimated the planet's diameter at about 3″, but in his letter to Le Verrier says it was not much to trust to, except under very favourable atmospheric conditions, and adds, "c'est principalement la carte qui a facilité la recherche." The place of the stranger was accurately determined by midnight, and again on the following evening, when it was found to have moved about 64 seconds of arc in the interim. From Dr Galle's letter it is also interesting to find that Bremiker's map was not the only new publication pressed into the service, as the final determining star was taken from the British Association Catalogue, which had just been placed in the hands of astronomers.

On October 1st, Professor Challis heard of the successful search at Berlin, and on turning to his notes, not only readily identified the new planet amongst the numerous stars which he had recorded nearly two months before, but also found that it had again been seen on September 29th, when, aided by a hint from Le Verrier's last paper, the observer singled out the planet from 300 stars, and appended to it the note "it seems to have a disk." The next night was cloudy at Cambridge, and, as has just been said, the news of the discovery came the following day.

It is not too much to say that the whole world rang with these tidings, but for a moment it seemed as if a painful international rivalry might arise as to the relative merits of the two great mathematicians to whom science owed one of her grandest triumphs. Better counsels, however, prevailed; and with an impartiality that will ever be regarded with satisfaction, the Testimonial of the Royal Astronomical Society was awarded to Le Verrier as well as to Adams in 1848. The Institute of France made Adams one of its corresponding members, as did also the Academy of Sciences of St Petersburg and many other societies. From Oxford he received the honorary degree of D.C.L., and that of LL.D. from Dublin and Edinburgh. He was elected an Honorary Fellow of this Society in 1849.

In 1851, Adams became President of the Royal Astronomical Society, to which position he was again elected in 1874. In 1858, he was appointed Professor of Mathematics in the University of St Andrews, but he returned to Cambridge in the following year, to take up the Lowndean Professorship of Astronomy and Geometry, which he held until his death. Three years later, he became Director of the University Observatory. Apart from many observations of planets, comets, &c., the zone $+ 25°$ to $+ 30°$ of stars down to the ninth magnitude was observed under his superintendence with the Cambridge transit-circle as a component part of the great international work set on foot by the Astronomische Gesellschaft. The actual observations are all finished and the reductions far advanced.

The Gold Medal of the Royal Astronomical Society was awarded to Professor Adams in 1866 for his researches on the moon's parallax and acceleration. After the great display of the Leonid meteors in the same year, Adams undertook the difficult task of determining their period. The researches of Professor H. A. Newton, of Yale College, had already shown that they must move in one of five definite orbits, but the difficulty was to decide which of them they followed. Here, again, Adams invoked the perturbations to solve the problem, and as the result of a most profound investigation, showed beyond all doubt that the periodic time of the meteors is $33\frac{1}{4}$ years. This orbit, it is scarcely necessary to add, closely resembles that of Comet 1866, I., as was first pointed out by Professor C. F. W. Peters.

Professor Adams communicated 43 papers to scientific societies, according to the Royal Society's Catalogue. To the Nautical Almanac he contributed valuable tables of the moon's parallax, and a Continuation of Damoiseau's Tables of Jupiter's Satellites. His classical *Explanation of the Observed Irregularities in the Motion of Uranus* appeared in the Appendix to the Nautical Almanac for the year 1851.

Professor Adams died at Cambridge Observatory on January 21st, 1892, after having been more or less an invalid for fully two years. Those who knew him most intimately cannot sufficiently express the profound impression made on them by his great gentleness and unassuming manner.

## Professor William Dittmar. By Professor Crum Brown.

(Read June 19, 1893.)

William Dittmar was born at Umstadt, near Darmstadt, 15th April 1833. He was the second son of Fritz Dittmar, then Assessor at Umstadt, afterwards Landrichter at Ulrichstein in Ober-Hessen, where his attitude towards the revolutionary party, in 1848, led to his retiring on a pension. He removed to Darmstadt, where William was apprenticed to the "Hof-Apotheker." After passing the "Gehülfe Examen" with distinction, he went to Mühlhausen, where for several years he served as assistant. He returned to Darmstadt for the Staats-Examen in Pharmacy, which he passed with distinction.

He then went to Heidelberg to work in Bunsen's laboratory, where he was soon appointed assistant. Sir Henry Roscoe invited him to Manchester as his private assistant, and, on his appointment as Professor of Chemistry in the Owens College, took Dittmar with him as assistant. In 1861 he became chief assistant in the Chemical Laboratory of the University of Edinburgh, under Lord Playfair. In 1869 he went to Bonn, where he acted first as "privat-docent," and afterwards as Lecturer on Meteorology at the Agricultural College at Poppelsdorf. In 1872 he declined the offer of the Chair of Chemistry in the Polytechnic School at Cassel, and returned to his old post in Edinburgh. In 1873 he was appointed Lecturer on Practical and Technical Chemistry in the Owens College, and in 1874 succeeded Professor Thorpe as Professor of Chemistry in Anderson's College, Glasgow. This office he held till his death, 9th February 1892. On that morning he lectured, but not feeling very well, went home in the middle of the day, and, after a few hours' illness, died at 11.30. He was a Fellow of this Society since 1863, and of the Royal Society of London since 1882. In 1887 the University of Edinburgh conferred on him the degree of LL.D. In 1891 the Philosophical Society of Glasgow awarded him the Graham medal for his investigation into the quantitative composition of water.

Dittmar was a good all-round chemist. His discovery of glutaric acid showed that he was quite at home in organic chemistry; but it was as an analyst that he was great. His investigation into the compositions of the specimens of sea-water collected by the "Challenger" Expedition is full of instruction in the way such work should be done. And in all his analytical work, and in all his teaching, his aim was not so much to perfect or to teach methods of analysis, as to settle and teach principles from which methods can be deduced as they are wanted. He was quick to detect sources of error, and estimate their effect on results. As the great instrument of the analyst, the balance early attracted his attention, and some of the most important improvements in the construction of the balance are due to him. Besides the work already mentioned on the quantitative composition of water, and on the composition of sea-water, the determination of the atomic weight of platinum, and the examination of the hydrates, carbonates, and peroxides of the alkali metals may be specially noted. Very interesting also are his investigations into the relation of the composition of acids of constant boiling-point to the pressure under which they are distilled.

Dittmar was an admirable teacher. He communicated to his students something of his own love of accuracy, and, instead of merely telling them what to do, and seeing that they did it, he also taught them to think for themselves. The transparent simplicity of his character, and the honest frankness of his manner, made his friendships close and constant rather than numerous.

## William Forbes Skene, LL.D., D.C.L., Historiographer-Royal for Scotland, &c., &c. By Professor Mackinnon.

(Read January 15, 1894.)

Among the notable Scotsmen of the nineteenth century, William Forbes Skene will have a foremost place. The author of *Celtic Scotland* was born at Inverie, in Kincardineshire, on the 9th of June 1809, and died in Edinburgh on the 29th of August 1893. His father was James Skene of Rubislaw, a member of an old Aberdeenshire family. His mother was Jane, daughter of Sir William Forbes, Baronet of Pitsligo. The family connections were numerous, and the future historian had, from earliest boyhood, unusually favourable opportunities of coming in contact with the men and women who in his day bore a worthy part in the life and literature of Scotland. His father was an intimate friend of Sir Walter Scott, and the son was more than once a welcome guest at Abbotsford.

Mr Skene was educated at the High School of this city, and afterwards attended the Universities of St Andrews and Edinburgh. Destined for the legal profession, he was apprenticed to a relative, Sir Henry Jardine, W.S. He became a member of the Society of Writers to the Signet in 1831, and was, for the long period of sixty-one years, a well known and honoured citizen of Edinburgh. He held an appointment in the Court of Session for many years, becoming latterly Depute-Clerk of the Court. In the meantime he had become the head of a prominent legal firm, a position which he retained till his death. In the public life of the city, whether political or municipal, Mr Skene took little part. A capable man of business, who had devoted attention to financial matters, he acted as Director of the Commercial Bank for over a quarter of a century. His philanthropic spirit and intimate acquaintance with the country and people, made him a most valuable Secretary to the Committee which administered the funds collected to relieve destitution in the Highlands and Islands after the failure of the potato crops in 1846 and subsequent years. Mr Skene gave much time to this work, and wrote valuable reports describing the operations of the Committee. In the education of the people he was deeply interested. He served for a couple of years on the School Board of Edinburgh, devoting special attention to that branch of the Board's work which was directed to the education of destitute and neglected children. For many years he taught a Bible class on Sabbath, for which full notes were carefully prepared. Some of these he afterwards wrote out in connected form, and published under the title of *The Gospel History for the Young.* A member of the Episcopal Church, Mr Skene was connected with St Vincent's congregation, of which he was for many years the main prop and stay. St Vincent's belonged to what was called the English Episcopal Church in Scotland; but, mainly through the influence of Mr Skene, the congregation became united some years ago to the Scottish Episcopal Church.

A man of many gifts and graces—an accomplished linguist; a well-equipped theologian, specially conversant with the development of doctrine and ritual; a proficient in music; a good talker, with a fund of anecdote, and not destitute of humour—Mr Skene was known among his friends. In literature he will be remembered as the most profound student of this century of the history and institutions of the early inhabitants of Scotland. Capable men toiled in the same field before Mr Skene's day. Not to go further back than last century, there were the able, if prejudiced Pinkerton, the dispassionate Innes, the erudite Chalmers. Within the last fifty years, valuable additions to our stock of knowledge in one department or another of the subject have been made by such men as John Stuart, Joseph Robertson, Bishop Forbes, Dr M'Lauchlan, Sir Daniel Wilson, Dr Reeves, Dr Joseph Anderson, and others. Mr Skene entered into the labours of these and such men. All that was worth reading, and much that was not, of what predecessors and contemporaries wrote, he knew thoroughly.

His own work combined that of the pioneer and the settler. He explored the ground and tilled it. Mr Skene was fully alive to the supreme importance of the evidence supplied by the concrete facts of anthropology and archæology, where such exist; but his own special labours lay in the departments of history and literary criticism. Very early he appreciated the truth that an original authority was foremost in value as in time. The first point was to ascertain what precisely the old authors wrote. With respect to many of them—the Roman writers, for example—once their exact words were known, the main difficulty was overcome. One had a reasonable guarantee that the fact was stated as these men saw it, the report as they heard it or read it. They were, as a rule, disinterested, and they must be regarded as on the whole trustworthy witnesses. With respect to native annalists, things were different. In the case of many of them the real difficulty may be said to commence after the accuracy of the text is established. Scottish historians had hitherto treated these authorities in one of two ways. They accepted them or rejected them *en bloc.* But this was surely unwise. The most imaginative among them occasionally writes history; the most prosaic frequently indulges in fiction. The task of the critic is to separate the fact from the fancy. For this purpose, accurate texts are indispensable; but accurate texts alone are not sufficient. The native chronicler must, at every step, be cross-examined and compared, not merely with himself, but as far as possible with contemporary writers, native and foreign. It is only when reliable material is thus obtained that the labour of the historian proper commences.

Mr Skene was, in many ways, exceptionally fitted for the work which he took in hand. A busy man all his life, he still could command leisure. He had a vigorous intellect, a powerful memory, a judgment in the main calm and clear. He possessed, in no small measure, the constructive faculty that was able to fit together into one reticulated whole isolated facts gathered from many quarters, the historical imagination that could clothe the dry bones with flesh and skin, and make the dead past live again. One most essential qualification Mr Skene possessed to which none of his predecessors could lay claim. Important light is thrown on many points of early Scottish history by Norse Saga and Welsh Triad. But, apart from the Roman period, the great mass of material is to be found in the works of native authors, written in old Gaelic or in Latin. Mr Skene's predecessors, as matter of course, could all read Latin, and one or two of them may have acquired a smattering of Welsh and Norse. But, among Scottish historians of the first rank, he was the only one since the days of George Buchanan who was able to read a Gaelic manuscript. While a growing lad his health was delicate, and, on the advice of Sir Walter Scott, his father sent him for a season to reside with the Rev. Dr Mackintosh MacKay, minister of Laggan in Badenoch, an accomplished gentleman, and, at the time, one of the most scholarly Gaelic students living. Under this competent guide young Skene studied modern Scottish Gaelic, a step which very probably shaped the future course of his intellectual history. In after years he extended his Celtic researches, not merely to the sister dialects of Irish and Manx, but to the kindred Brythonic tongues, especially Welsh. When, in 1853, Zeuss opened up in his *Grammatica Celtica* the old forms of the Celtic dialects to the world, Mr Skene entered upon the study of old Gaelic with ardour. His previous training, combined with his command of French and German, enabled him to follow with ease the development of Celtic studies abroad by Ebel, Schleicher, Windisch, Gaidoz, D'Arbois, Loth, and others. He was one of the very few in Scotland who bought and read the continental magazines devoted to linguistic, and largely to Celtic studies—the now defunct *Beiträge zur Vergleichenden Sprachforschung*, the *Zeitschrift für Vergleichende Sprachforschung*, and the *Revue Celtique.* For some years back Celtic scholarship in the hands of Stokes, Zimmer, Thurneysen, Rhys, and others has occupied itself largely with sounds and forms and accents—the blood, bone, and muscle of grammar. Here a good grounding in phonetics, a minute verbal study of texts, and an acquaintance with living dialects are essential. Perhaps Mr Skene was not in full sympathy with this latest development in Celtic philology. He ceased to buy *Kuhn's Zeitschrift* many years ago, and even the *Revue Celtique* was dropped by him in 1887. Throughout several of his published works, one observes now and again a failure on the part of the author to grasp the subtleties of Gaelic and Welsh grammar. One example must suffice. The title of his great work is *Celtic Scotland: a History of Ancient Alban.* In a note (*Celtic Scotland*, vol. i. p. 1), the author explains how he adopts the genitive *Alban* in preference to "what he ventures to call the pedantic affectation of using the form

Alba," which is the old nominative. "A nominative form derived from the genitive is," he says, "also found; and the names of places ending in a vowel seem to have a tendency to fall into this form in current speech." In Gaelic, as in other tongues, an oblique case, through the loss of flexion, is frequently raised to the nominative. The particular case selected for this preference is that most frequently heard in current speech, and in place names the case raised to the nominative is in Gaelic always the locative-dative,—the genitive never. In this particular instance, the choice lies not between the old nominative *Alba* and the old genitive *Alban*, but between the old nominative *Alba* and the old dative *Albin*. The fact is, *Alban* and *Aran* are, like *Iona*, names invented for use in books and maps only. The correct forms, like *Erin* and *Rathlin*, are *Albin* and *Arin* (in Gaelic orthography *Albainn*, *Arainn*).

When Mr Skene took up his abode permanently in Edinburgh, he and several gentlemen interested in Celtic literature and history founded the Iona Club. The club came to an end on the death, in 1837, of one of its most active members, the late Donald Gregory, W.S., author of a valuable *History of the Western Highlands and Isles of Scotland*. The club published but one volume of *Transactions*, a book now rarely met with, the *Collectanea de rebus Albanicis*, in which are printed extracts from family charter-chests, national records, Gælic MSS., Irish annals and Norse sagas, which throw valuable light on the history of the tribes and clans of the North of Scotland. In these early years the Highland Society of London offered a premium for "the best History of the Highland Clans." Mr Skene competed, and his essay proved the successful one. It was afterwards enlarged and published in two volumes in 1837. *The Highlanders of Scotland*, as this work is entitled, is now a somewhat rare book. With the confidence of youth, Mr Skene states that in the preparation of this essay he had given a long and attentive examination to the early authorities in Scottish history, and had thoroughly investigated two new and most valuable sources—the Icelandic Sagas in their original language, and the Irish Annals. The author afterwards found cause to modify several of the views advanced in this early work; but in its main features the juvenile production is characterised by the same qualities which distinguish the writer's maturer labours—fulness of information, clearness of statement, soberness of judgment, and a dignified courtesy which ever ruled the pen as well as the speech and bearing of Mr Skene.

It was in 1859 that Mr Skene became a member of the Royal Society. A valuable paper by him, afterwards printed in *The Four Ancient Books of Wales* (vol. i. p. 141), on "The Celtic Topography of Scotland, and the Dialectic Differences indicated by it," was read before the Society in 1865, and printed in vol. xxiii. of its *Transactions.* He joined the Society of Antiquaries in 1831; became a vice-president of that Society in 1852; and was throughout a frequent contributor to its *Proceedings.* Papers from his pen on linguistic, literary, genealogical, and historical subjects appear frequently from 1852 till 1886. Some of the early papers—*e.g.*, "On Ancient Gaelic Inscriptions in Scotland" (*Proceedings of the Society of Antiquaries of Scotland*, vol. i. p. 81), "On the Ogham Inscriptions on the Newton Stone" (*Ibid.*, v. 289)—are now superseded. Others—*e.g.*, "The Earldom of Caithness" (*Ibid.*, xii. 571) and "The Authenticity of the Letters Patent said to have been granted by King William the Lion to the Earl of Mar in 1171" (*Ibid.*, xii. 603)—are reprinted as appendices in *Celtic Scotland* (vol. iii. 441, 448). An elaborate treatise on "The Coronation Stone" (*Proc. Soc. of Ant. of Scot.*, viii. p. 68) was afterwards published separately. The greater number of these valuable papers are special studies on obscure points in Scottish history and bibliography, the conclusions arrived at being, as a rule, accepted as established in the author's more elaborate works.

The services of Mr Skene to Celtic history and literature may well be termed great. The fact that a man of his ability and culture set himself resolutely to study the Celtic dialects as an essential preliminary to the investigation of the history of the tribes who spoke these dialects, gave an importance and a distinction to these studies which, in this country, they much needed but did not always receive. He cannot, indeed, be said to have expelled the linguistic charlatan from his chief stronghold on European soil. We have still among us educated men who will undertake to explain obscure Gaelic names without learning to decline a Gaelic noun, and to correct Highland maps though they cannot spell a Gaelic word.

To Mr Skene is due the credit of bringing together the very valuable collection of Gaelic MSS. now deposited for preservation and reference in the Advocates' Library, Edinburgh. The Faculty of Advocates themselves possessed four such MSS. When a committee of the Highland Society of Scotland (now the Highland and Agricultural) undertook to conduct an inquiry into the authenticity of Ossian's Poems, Gaelic MSS. were sent by the Highland Society of London and others, with the view to aid the committee in their labours. The late Major M'Lachlan of Kilbride was the possessor of a considerable number of such MSS.; these had disappeared, but were eventually found in Glasgow. Through Mr Skene's representations the Highland Society and the custodier of the Kilbride MSS. agreed to deposit their collections in the Advocates' Library. A few others were added, and a general catalogue of the whole was prepared by Mr Skene.[1] One of these Gaelic MSS. consists of a large collection of verse made by James MacGregor, Dean of Lismore, in the early part of the sixteenth century (Adv. Lib. Coll., xxxvii.). The late Dr M'Lauchlan of Edinburgh transcribed, translated, and annotated large extracts from this manuscript, which were published under the title of *The Book of the Dean of Lismore* by Edmonston & Douglas, of this city, in 1862. To this volume Mr Skene contributed valuable notes, and an elaborate introduction on the history of Gaelic, and especially Ossianic, literature. In 1868 appeared the *Four Ancient Books of Wales* in two large volumes, published by the same Edinburgh firm. The second volume contains copious extracts from the poems in the Black Book of Carmarthen, the Book of Aneurin, the Book of Taliessin, and the Red Book of Hergest, with notes, appendices, and index. The first volume gives a translation of the Welsh poems into English by the Rev. D. Silvan Evans and the Rev. Robert Williams, with an elaborate introduction by Mr Skene, embracing chapters on the Races of Britain, the Celtic Dialects, the Pictish Language, the Celtic Topography of Scotland, as well as on the Ethnology and the early Literature and History of Wales.

Mr Skene's contributions to the history proper of Early Scotland

[1] In course of time, Skene became himself the possessor of several Celtic MSS., which he bequeathed to the Advocates' Library collection, but unfortunately those of most value seem to have disappeared.

are of a two-fold character. There is first of all the collecting, sifting, and arranging of the raw materials for such a history; and there is besides the stately pile which he himself constructed out of these materials. In addition to the papers already spoken of, contributed to the *Transactions of the Iona Club* and the *Proceedings of the Society of Antiquaries*, falls to be mentioned a translation, with introduction, notes, and illustrations, of John of Fordun's *Chronicle of the Scottish Nation*, a work which forms vols. i. and iv. of the *Historians of Scotland* series. Vol. vi. of the same series is an adaptation of Dr Reeves's great work, *Adamnan's Life of St Columba*. In the Scottish edition of this monumental book a translation of Adamnan's Latin text by the late Bishop of Brechin is given, while Dr Reeves's learned and exhaustive notes are condensed and recast by Mr Skene. The most important contribution of this description to Scottish history made by Skene is the large volume known as the *Chronicles of the Picts and Scots*, edited by him, and published under the direction of the Lord Clerk-Register of Scotland in 1867. In addition to the *Chronicle of the Picts* and the *Chronicle of the Scots*, there are here gathered together "as complete a collection as possible of the fragments which still remain of the *Early Chronicles and Memorials of Scotland*, prior to the publication of Fordun's *History*." The extracts written in Saxon, Welsh, and Gaelic are accompanied by a translation; those written in Latin are left untranslated. In all, fifty-eight documents, in whole or in part, are printed, with a preface extending to nearly 200 pages of large octavo, giving a description and examination of the documents. "The first piece, both in point of time and of importance, is that usually known by the name of the *Pictish Chronicle*." It is in three parts, and Mr Skene is of opinion that the second and third divisions have been translated from an old Gaelic original by a scholar who did not always understand his text. A Gaelic word or phrase is occasionally left untranslated, *e.g.*, "Athelstan filius Advar *rig Saxan*,"—*rig Saxan* being Gaelic for "King of the Saxons." The editor concludes that the Chronicle proper was written originally in Gaelic at Brechin between the years 977 and 995.

Mr Skene's reputation as an historian will rest on his *chef-d'œuvre, Celtic Scotland*. This important work, the outcome of over forty years' study and research, was published in three volumes by David Douglas, Edinburgh, 1876–80. Each volume is practically an independent work in itself. The first treats of the early races of Scotland, and records the civil and political history of the various peoples down to the death of Alexander III., when the purely Celtic dynasty became extinct. The second volume is entitled "Church and Culture." The ecclesiastical history closes with the twelfth century, when the old Celtic Church came to an end, and in Scotland Columbanism gave place to Romanism. A single chapter on the language and learning of the people gives in outline the leading facts in the literary history of the Scottish Gael to the middle of last century. The title of the third volume is "Land and People." Here the attempt is made to picture the social life of the tribes in early times, and of the Highland clans down to our own day; the relation of the various classes to each other; their land tenure, mode of agriculture, privileges, and exactions.

The outstanding features of this great work are the fulness and accuracy of the author's knowledge, and the conspicuous fairness with which facts are grouped and conclusions drawn. The style, one cannot help thinking, is to a certain extent coloured by the profession of the writer. The reader will look in vain here for the stately periods of Gibbon, still less for the brilliant rhetoric of Macaulay. Mr Skene's style is always dignified, occasionally rising to eloquence. But, in reading his pages, one is rather reminded o a memorial for counsel drawn by a masterly hand, the relevant facts all marshalled with skill, and justifying the "Opinion," which is always argued with ability, and not infrequently with ingenuity and subtlety.

The plan of the work is not without its disadvantages. The civil and ecclesiastical cannot always be kept separate. The ethnological chapters of vol. i., and the discussion on the legendary origin of tribes and clans in vol. iii., necessarily overlap. The early history of Scotland presents many difficult problems; but the most insoluble are those dealt with by Mr Skene in his third volume. The social and domestic life of the Pict is practically unknown. A glimpse is given in the Book of Deer; all else is dark. There *mormaer* and *toisech* and chief of clan appear, subordinate to the king who is supreme, and with inherent though undefined rights in the soil. That the *mormaer* became eventually the earl, and the *toisech* the thane (chief and captain of clan among the modern Highlanders) seems to be established. But the relationship of these dignitaries to each other, to the king, and to the mass of the people; their mode of life; their beliefs; their judicial system, are shadowy in the extreme. The materials for filling in the vague outline given in the Book of Deer are to be gathered from stray notices and allusions in native records, but chiefly by comparison and analogy from Irish, Welsh, Saxon, and Norse sources, and these have by no means been exhausted by Mr Skene.

The case is different with respect to the ecclesiastical history of these people. The chapter on the literature of the Scottish Celt is meagre; but the history of the old Church of Scotland, as written by Mr Skene, is full and reliable. Additional facts are daily coming to light; but the main conclusions arrived at in this volume are not likely to be materially shaken. In this field the record is fuller, and the author was perhaps more in sympathy with his subject. Vol. ii. of *Celtic Scotland* is virtually accepted as authoritative, being quoted as such by writers of various creeds. The mission of Nennius is overshadowed by that of Columba. The early Scottish Church is essentially Columban, an offshoot of the Church of Ireland. The creed, organisation, and discipline of the old Church of Scotland have been the subject of hot controversy. The Apostle of the Picts was, as Bede says, a presbyter. Mainly because of this fact, some have held that the Columban Church was Presbyterian. There were bishops in Iona in early days. To that extent, at least, the old Gaelic Church in Scotland was Episcopal. But these bishops had no dioceses, and in the monastery they were under the jurisdiction of the abbot, who was supreme. One thing is clear. The early Church of Ireland and of Scotland was not Roman. During the fifth and sixth centuries, the Church in the British Isles was in practical isolation from the Church abroad. Considerable differences had meanwhile emerged. But when Columbanus came into collision with the bishops in France, instead of adopting their views on the matters in dispute, he stoutly asserted his independence both of them and of Rome; and in the great conference at

Whitby, Colman of Lindisfarne upheld the authority of St John and St Columba as equal to that of St Peter and the Pope. Mr Skene shows clearly that the early Gaelic Church was not Presbyterian, Episcopal, or Roman, as we understand these terms nowadays. It had at least two distinguishing and praiseworthy features: it combined great missionary zeal with literary enthusiasm. Several ideas and practices, among them its intense monasticism and the passion for an eremitic life, the old Gaelic Church, by ways and channels which we do not as yet fully know, borrowed from the East. Its most peculiar feature was the manner in which the tribal organisation of the Gael was adapted to the government and discipline of the monastery. The headship of the tribe or clan was as to family hereditary; but, in theory at least, elective as to the individual. In Iona the bishop, though often spoken of, was a subordinate person; the abbot was all and in all. The abbot of the monastery was, like the head of the clan, selected out of the family of the person who founded the monastery. This peculiar arrangement took root and prospered among the Gael. The idea was native, and very probably it helped to make the Gaelic Church so intensely national, or, more properly speaking, racial. By the beginning of the thirteenth century the Columban Church was externally extinct in Scotland. It is to be regretted that the limit which Mr Skene had imposed upon himself precluded him from inquiring to what extent, if any, the old ideas and ways survived under the Roman organisation that displaced them. A chapter from his pen on the Highland Church from the thirteenth to the sixteenth century would be a valuable contribution to the ecclesiastical history of Scotland.

The civil and political history of Scotland in early times is written by Skene with a fulness hitherto unattempted. The portion of Scotland conquered by the Romans was held but for a limited period, and upon a precarious tenure. After the withdrawal of the legions the thick darkness that followed is broken by the landing in Argyleshire of a colony of Irish Gaels in 503. During the next 350 years four peoples struggle for the mastery on Scottish soil—the Gael of Dalriada, the Britons of Strathclyde, the Saxons of the south-east, and the Picts, who lived beyond the Forth, and, according to Skene, in Galloway. Eventually these races were so far consolidated under Kenneth MacAlpin, a Dalriad, whose line, amid many vicissitudes, held the throne of Scotland till the close of the Celtic period. In his chapters on ethnology Skene discusses the language and race relationship of the Picts, and comes to the conclusion that they were Celts of the Gaelic rather than of the British type. The view advanced by Pinkerton, and upheld by Oldbuck of Monkbarns, that these people were Teutons who spoke a Gothic dialect, is now exploded. It does not follow that Mr Skene's must be accepted. Within the last few years Professor Rhys, with great learning and no small ingenuity, has argued that the Picts were of Turanian stock, whose speech was largely overlaid by loans borrowed from their Gaelic and Brythonic neighbours. Mr Skene's proof is mainly linguistic, and is two-fold. If the ancestors of the Northern Highlanders spoke a language other than Gaelic, some remains of it would have survived. Again, only on two occasions is Columba spoken of as using an interpreter when preaching to the Picts, the inference being that, as a rule, the saint's Gaelic speech must have been understood by these people.

The problem cannot, however, be solved on such narrow issues as these. The questions of blood and language must always be kept distinct. Anthropology and archæology may hereafter yield concrete evidence which will be decisive of the matter. As things are, the following facts must be kept in the fore-front. Among the Picts, succession was through the female. This custom is unknown among Celts; it is indeed, so far as we know, non-Aryan. Again, Bede regarded Pictish as a separate language. The Gael of Ireland and Scotland looked upon the Picts or Cruithnig, to use the native term, as a people different from themselves. Cormac, the first Gaelic lexicographer, gives one or two Pictish words, quoting them as foreign words, at a time when, presumably, Pictish was still a living language. The Norsemen called the Pentland Firth Pettland, *i.e.*, Pictland Fjörd, while the Minch was Skottland Fjörd. Mr Whitley Stokes, after examining all the words in the old records presumably Pictish, says: "The foregoing list of names and words contains much that is still obscure; but on the whole it shows that Pictish, so far as regards its vocabulary, is an Indo-European and especially Celtic speech. Its phonetics, so far as we can ascertain them, resemble those of Welsh rather than of Irish."

This splendid record of good work done was duly acknowledged at home and abroad. As was natural, Celtic societies in Ireland and Wales, as well as in Scotland, felt pride in honouring the distinguished historian. The University of Edinburgh conferred the degree of LL.D. upon Mr Skene in 1865. He became D.C.L. of Oxford in 1879. Upon the death of Mr Hill Burton in 1881, the Government of the day, with the full approval of educated Scotsmen of all classes and creeds, made him Historiographer-Royal for Scotland. And in 1888, friends and admirers commissioned Sir George Reid, now President of the Royal Scottish Academy, to paint his portrait; the portrait to remain with Mr Skene during his lifetime, and at his death to be sent to the National Portrait Gallery.

## Alphonse Louis Pierre Pyramus de Candolle.
## By Professor Frederick O. Bower, F.R.S.

(Read February 19, 1894.)

It has happened not uncommonly in the science of Botany that more than one generation of a family has followed the same pursuit. The subject of this notice was the second notable botanist of his name, and he leaves a son who also pursues the same science.

Augustin Pyrame de Candolle, the father of Alphonse, sprang from a Provençal family, which had fled from France in 1558 to escape religious persecution, and had settled in Geneva. He appears to have spent his earlier years in Paris, where he was intimate with the leading men of science; subsequently he held the chair of botany at Montpellier; but in 1814 he finally took up his residence at Geneva, having been appointed to the chair of botany in his native city. Himself a man of surprising powers of application, he set on foot that great work of descriptive botany, the *Prodromus Systematis Naturalis*, in which it was intended that all known plants should be arranged according to a natural system, and described at length. It was into this great enterprise that Alphonse de Candolle entered in early manhood, and at a time when his father was still actively at its head. It was to this that he devoted a great part of his long and strenuous life; at his death the great work remains still incomplete, though a wonderful monument of the capacity and endurance of two generations.

Born at Paris in 1806, Alphonse was still a small child when his father settled at Geneva. It might have seemed natural that, after the ordinary period of general education, he should, as the only son, take up the subject pursued by his father; but the latter, wishing him to enter a profession of more certain profit, directed him to the study of law, in which he graduated in 1829. But he had already in 1824 begun the long series of his botanical publications, which was continued till 1893; his inclinations seem plainly to have been towards the study of the laws of nature rather than of man, and, after the publication of some botanical notes of minor importance, we find him in 1830 as the author of his first work on systematic botany, a monograph of the Campanulaceæ. This included, in addition to the more purely systematic treatment of the family, a very complete statement of the facts relating to its geographical distribution, and thus it foreshadowed the work which the author was in later years to accomplish in the two spheres of purely systematic botany, and of botanical geography.

Alphonse de Candolle was for a considerable time officially connected with the University of Geneva. In 1831 he was appointed honorary professor, with the duty of assisting his father in the management of the Botanic Garden, as well as in academic affairs. In 1835 he was appointed ordinary professor in his father's place, a post which he held till 1850, when he retired from the exacting duties of teaching to labours in the more direct advancement of his science.

The *Prodromus*, already planned by Aug.-Pyr. de Candolle, had reached its seventh volume when Alphonse de Candolle began to participate in its production. From that point onwards he contributed largely from his own pen to the monographs, while after his father's death in 1841 the editorship of the great work was entirely in his hands. The whole series of 17 volumes (1824–1873) consists of 13,194 printed pages; of these Alphonse de Candolle contributed 1387 pages, dealing with 45 families, 438 genera, and 5511 species. Those who are acquainted with such work will from these figures form some estimate of the great area of observation and accurate description over which he must have spread his energies.

During the half century over which the publication of the *Prodromus* extended, botany had been steadily advancing, and the advance is reflected in the style of the writing put into it by de Candolle and his collaborators. The descriptions become less brief, and more attention is given to the geographical distribution of the species. It is true that comparative morphology, development, and anatomy do not figure largely, for such branches of the science were in their infancy at the time when the idea of *Prodromus* was conceived. It was inevitable that, in a work of which the publication of the first part was necessarily separated from the later by so long a period as half a century, the earlier parts should become obsolete before the work was completed, and no doubt in the original scheme a much more rapid progress was expected than actually proved possible. Be this as it may, it was at last found by Alphonse de Candolle that it was undesirable to attempt to complete the *Prodromus*, and in 1873 the work was finally closed, the Monocotyledons not having been even touched.

This unsatisfactory position has, however, been met by initiating a separate publication, under the title of the *Monographiæ Phanerogamarum*, of which the eighth volume is now in the press, the editorship having been shared by Alphonse and his son, Casimir de Candolle. The object of this work has been partly to revise the orders treated in the earlier volumes of the *Prodromus*, and secondly to take up the Monocotyledons, which were omitted from the *Prodromus*. A circular letter was issued in 1875 announcing the scheme and method of the new enterprise. Though well responded to, only seven volumes of the new work have yet appeared, including 17 families, eight of which are from the Monocotyledons. The treatment of the Smilaceæ in the first volume, by Alphonse de Candolle himself, showed the wideness of the new scheme; for he took into account the anatomy, the affinities, the geographical distribution, and the fossil representatives of the family.

Here it may not be amiss to mention the extensive collection brought together originally by the father, and continually growing under the management of the son. It is probably the largest private collection in existence, its rival having been the Hookerian Herbarium, now incorporated with the great collection at Kew. This, together with the drawings and library, all managed with the greatest perfection, was willingly placed at the disposal of visitors, and especially of those who were engaged as collaborators in the systematic undertakings of the de Candolles.

Working upon this extensive herbarium, among divers families, gave de Candolle an opportunity of extending the science beyond the mere recognition and description of new forms, an opportunity which he grasped from the first. It has already been remarked that in his earliest monograph of the Campanulaceæ he paid particular attention to the geographical distribution of the species.

Himself never an extensive traveller, he yet, by careful and systematic collection of facts, prepared himself to be the author in 1855 of the *Géographie botanique raisonnée*, which is considered to be his most important work. It was not his object to compile from books of travel a description of the vegetation of the earth, nor did he attempt to explain all the known phenomena of distribution of plants. In his own words his object was "to seek out the laws of the distribution of plants upon the earth, by means of a limited number of facts, which should serve as a basis, and proofs"; "*rerum cognoscere causas* should be the goal in all true science." And again, the principal object should be to show in the distribution of plants as they are, what may be explained by the actual conditions of climate, and what depends upon anterior conditions. The work was divided into three parts: the first dealt with the mode of action of temperature, light, and moisture upon plants; the second with plants from the point of view of their distribution on the globe, the causes of their origin, their frequency or rarity; in the third the different countries were studied from the point of view of their vegetation.

His introduction of a modified method of the sum of temperatures was perhaps the most important point. Boussingault had already introduced the method, calculating the sum of temperatures upon the rough thermometric mean. De Candolle showed that the true method of sums of temperatures consists in calculating them above a certain minimum, from which point the vital phenomena of the plant in question begin to be active. Each species extends further northwards as far as it finds a certain fixed sum of heat, thus calculated, between the day when a certain mean temperature commences, and that when it ends; but these rough results are modified by other conditions; still, though not mathematically exact, the method laid down by him gives useful results in connection with the study of the geographical limits of species.

These and kindred subjects occupied the attention of de Candolle repeatedly in later years; the most important of his later geographical writings being that in which he distinguished among plants six "physiological groups." In these were associated together plants which behave alike with regard to heat and moisture, and which accordingly may have together passed through different geological phases, and are always found in those regions of the globe where similar conditions occur.

Having thus interested himself in questions of geographical distribution at large, it seems a natural step in specialisation of such study that de Candolle should have taken up the question of the "origin of cultivated plants." The difficulties of this subject are not merely botanical, but ethnological, historical, palæontological, and even linguistic; he arrived at his conclusions by a combination of all these lines of research. The result of this wide research, involving such varied and numerous facts, was a book published in 1882, which takes its place as the first authority on the subject.

The attitude of de Candolle towards evolution was favourable from the first. Considering that he was already over fifty years of age when the *Origin of Species* appeared, it would have been conceivable that his opinions should have been too long held for change. But, on the other hand, his writings previous to it show that he was well prepared for some such view. He had already speculated upon the origin of those "physiological groups" mentioned above, and had included in his reasoning observations and ideas relating to earlier geological periods. He had even recognised the possibility of new hereditary forms, which should have been derived from actual specific forms; but he felt the difficulty of such modifications being brought about without the hand of man, there being little probability that these modifications would be transmitted in the ordinary course of things; still he admitted the possibility of species, under the influence of diverse circumstances, being modified, and developing accidentally under a new form. To one who was already in this position, "the origin of species, by means of natural selection," would be accepted as a welcome solution of the difficulty. He wrote in 1862, "Darwin has placed his finger upon the essential point of the question, by seeking a cause by which the variations from one generation to another would be necessarily fixed instead of disappearing"; while in 1873 he wrote, "One had believed in this evolution without understanding how it could operate; selection has come as an explanation how the changes, once produced, are fixed."

But it would be impossible here to review all the literary achievements of this most fertile writer; for almost seventy years he was at work, and the mere list of his publications, in which his contributions to the *Prodromus* and to the *Monographiæ* appear only as single numbers, amounts to 235. His botanical subjects ranged from strict taxonomy, through writings on geographical distribution, effect of external conditions on plants, and economics, to the theory and practice of botanical description and nomenclature. But, like many men of outstanding ability, his energies were at times diverted into other lines than those of his favourite study; bred a lawyer, he doubtless found that early training of value in his capacity as a member of the Representative Council, which he entered in 1834. Judging from his mixed writings, his interests appear to have been wide, with a special bias towards anthropology, and the amelioration of the conditions of the race; these tastes found their expression in his legislative successes.

It was natural that a man with such a scientific record as his should have received very wide recognition, not only in his own country, but throughout the scientific world. The Royal Societies of London, Edinburgh, and Dublin, the Institute of France, the Academies of all the chief capitals of Europe, claimed him as a foreign member or associate. Our own Society will feel that in offering him in 1877 a place among the foreign fellows it had honoured itself. He has gone to the grave full of years and of honours, leaving as his mark upon the progress of botany such a record of solid and long-continued work as has seldom been attained by scientific writers.

## Professor William Morse Grailly Hewitt.
## By Professor A. R. Simpson.

(Read March 5, 1894.)

Dr William Morse Grailly Hewitt, who was born on 3rd August 1828, matriculated with honours in Chemistry in the University of London, studied medicine in the University College, and, on graduating as M.B. in 1850, obtained honours in all the four branches of the final examination. His knowledge of the scientific departments was very complete, and he retained his acquaintance with Anatomy, Physiology, Botany, and Chemistry throughout his career. After studying for a time in Paris he settled in London, became associated with St Mary's Hospital, where for some years he acted as Registrar, and was Lecturer on Comparative Anatomy and Zoology.

In 1855 he took the higher degree of M.D. in London University, and the following year became a Member of the College of Physicians.

He had been alone among his compeers to win honours in Midwifery at his graduation, and he soon began to direct his activities mainly to the obstetric department of medicine, and was largely influential, along with Dr Tyler Smith, in founding the London Obstetrical Society, of which he was the first Secretary. This office he held for six years, when he became one of the Vice-Presidents, and afterwards President. In 1858 he was appointed Physician to the Samaritan Hospital for Women, and in 1859 Physician to the British Lying-in Hospital. He had already become Assistant Physician-Accoucheur to St Mary's Hospital, and Lecturer on Midwifery and the Diseases of Women and Children in the St Mary's Hospital Medical School. In 1865 he was called to fill the Chair of Midwifery in the University College of London, becoming at the same time Physician-Accoucheur to the University College Hospital, and Director of the Obstetrical Department. The duties of the professorship and its associated offices he fulfilled with assiduity and distinction for twenty-one years, during which he published a valuable work on the Diseases of Women, and many important memoirs, which placed him in the front rank of the obstetricians of his time, and led to his being elected as Honorary Fellow of the Obstetrical and Gynæcological Societies of Edinburgh, Berlin, Boston, Helsingfors, &c.

Whilst his most numerous and important contributions were made to Obstetrics and Gynæcology, Professor Grailly Hewitt wrote also some papers of permanent value and interest on Whooping-cough and other diseases of childhood, published a suggestive treatise on Nutrition as the Basis of Treatment in Disease, and more recently discussed, in two communications to *The British Medical Journal*, the subject of Visual Disturbance as a Cause of Sea-Sickness.

He became a Fellow of the Royal Society of Edinburgh in 1889. He had retired for some time from active service, and died in London on 27th August 1893, "in such peace," says the *Lancet's* obituarist, "that the beautiful lines of his well-known John William Inchbald seemed to be written as if for him—

'Is it deep sleep, or is it rather death ?
But anyhow it is, and sweet is rest ;
No more the doubtful blessing of the breath,
Our God hath said that silence is the best.' "

## Rev. Thomas Brown, D.D. By Professor Duns, D.D.

(Read March 19, 1894.)

Thomas Brown was born on the 23rd of April 1811, in the manse of Langton, Berwickshire, of which parish his father, the Rev. John Brown, D.D., was minister. Mr Brown entered the University of Edinburgh in 1826, and, at the close of his Arts course, was enrolled as a student of divinity. His academical record was that of an able and diligent student, who gave himself earnestly to the work of the classes, and took a lively interest in more than one university debating society. Mr Brown was licensed as a probationer of the Church of Scotland in 1835, and in 1837 was settled as parish minister of Kineff, Presbytery of Fordoun, Aberdeenshire. At the Disruption of the Church in 1843, Mr Brown joined the ministers and laymen who then formed the Free Church. In 1848 he was married to Miss Wood, a member of an old and well-known Edinburgh family.* In 1849 he accepted a call to be minister of the Dean Free Church, Edinburgh, and in this position made thorough proof of a ministry solid, full of instruction, and withal attractive.

Mr Brown was elected a Fellow of this Society in 1861. In

* On her father's side, Miss Wood was connected with the Woods of Warriston, and on her mother's with those of Largo. Towards the opening of the present century, the former was represented by a popular physician, who figures in Kay's Edinburgh Portraits as "Lang Sandy Wood," but was also known by a kindlier name. In a clever parody of "Childe Harold," which appeared in *Blackwood*, May 1818, beginning, "I stood, Edina, on thy Bridge of Sighs," we have the following lines:—

> "Munro once ruled and Gregory now reigns;
> George Bell now feels the pulse which John Bell felt.
> Dispensaries, infirmaries, and chains
> Purge, slash, and clank, where'er the cities belt
> Girdles it in—a space that may be smelt!
> So we go on, I fear to little good,
> Meanwhile the rivals one another pelt!
> Oh for one hour of him who knew no feud,
> The octogenarian chief, the kind old Sandy Wood!"

It's pleasant to gather up any separate link like this, and give it a place in the genealogical chain.

1888 he received the degree of Doctor of Divinity from the University of Edinburgh. In 1890 he was called to the Moderator's Chair of the Free Church General Assembly, which he occupied with dignity, and with much satisfaction to the Church. Dr Brown died on the 4th of April 1893. Two sons survive him—J. Graham Brown, Esq., M.D., and J. Wood Brown, M.A., minister of the Free Church, Gordon, Berwickshire. His brother, Sir John Campbell Brown, K.C.B., a highly distinguished member of the Indian Medical Service, predeceased him.

Let this bald and rapid enumeration of the leading family and public steps in Dr Brown's life serve as introductory to what, in the obituary notice for the *Proceedings* of the Society, is of chief interest. I refer, mainly, to the records of the work he has done as one of its Fellows. This work may be looked at under three divisions:—Geology, Botany, and Literature.

I. *Geology.*—In comparatively few districts of lowland Scotland could a youth with an inborn bent towards natural science have found fuller scope for observation and research than in that part of Berwickshire in which Brown was born, and in which he spent his youth. The environments do not make the man, or determine his tastes, but much of a life depends on correspondence between natural bent and surroundings. The latter is ever at hand to develop, to cherish, and to strengthen, without perfectly satisfying, the former, and thus to allure to ever higher effort. The geological and botanical features of Langton parish, and other neighbouring parishes, are full of interest. Within little more than a gunshot from the manse, the Lower Carboniferous strata crop out in the Langton Burn course, with their embedded ichthyolites and remains of plants. In the same burn course are strata which seem to mark the meeting-place of the Carboniferous and the Old Red Sandstone, while, in near localities, are shales and clays yielding remains of other plants, mollusca, and fishes. And by a walk of a few miles he could reach what Hugh Miller describes as "The deep belt of Red Sandstone which leans to the south (in the valley of the Whiteadder) against the graywacke of the Lammermoors."

While avoiding details, it seems to me that a brief statement of the character and scope of his chief contributions to geology appropriately fits into this sketch of his life and work.

1860. His first paper is singularly free from the defects which generally characterise first attempts in the literature of any branch of science. It is entitled, "Notes on the Mountain Limestone and Lower Carboniferous Rocks of the Fifeshire Coast from Burntisland to St Andrews." This paper was read in April 1860, and printed in volume xxii. of the *Transactions.* Mr Brown had gone to Elie in the autumn of 1856 for a few weeks' rest, and, he says, was induced to pay some attention to the geology of the district, resuming, for a brief interval, what was once a favourite pursuit. His ever active habit of the eye had its reward. A thin bed of limestone, dipping inland from the shore, caught his attention. Ichthyolite, molluscan, and crustacean remains were found in it, and as some of these were well-known Irish forms, they raised the question,—May not this bed of limestone synchronise with the Irish series in which these forms occur? Mr Brown felt he had broken new ground here, because neither M'Laren, nor Landale, nor Anderson, who had worked much in the neighbourhood, had referred to it. He resolved, in the face of many difficulties, to work it out, and for several years devoted his autumn leisure to this. He succeeded, both from the stratigraphical and palæontological points of view.

1863. "On a Clay Deposit, with Fossil Arctic Shells, recently observed in the Basin of the Forth." This bed of clay was discovered, and the attention of geologists first called to it, by Mr Brown. It was specially interesting to him at the time as, he thought, indicating the former existence in Scotland of an Arctic climate—the shells found in it being for the most part exclusively Arctic, and several of them new to British glacial deposits. He believed, moreover, that the stratigraphical position of this bed warranted the inference of a considerable rise throughout the whole seaboard of the Forth.

1864. "Notice of Glacial Clay, with Arctic Shells, near Errol, on the Tay." The shells in the Errol brick-clay were found to be identical with those at Elie. The area within which these shells occur thus became greatly enlarged, and, as he thought, it also favoured his theory touching the rise of the land.

1874. "On the Parallel Roads of Glenroy," Lochaber. The subject has proved a tempting one to students of quaternary deposits. The theories of their formation were mainly three:—(1)

The Maculloch, Dick-Lauder, Milne-Home theory—the glen once the site of a lake; (2) The Darwin, Nicol, R. Chambers theory—the terraces mark the level of an arm of the sea at three different periods; and (3) The Agassiz, Buckland, (Mr) Jamieson theory—glacier lake; the glacier, melting at three widely separated periods, left the marks of this in the terraces.

Dr Brown approached the problem from a new (the biotic) point of view. That the deposits contain no shells was accounted for by Darwin alleging that the carbonic acid gas in the rain-water had destroyed the shells. Mr Brown, remembering that the so-called shells of diatoms, being siliceous, would not be destroyed by this gas, resolved to search for diatoms in the terrace deposits, and diatoms were found which Professor Dickie of Aberdeen, an acknowledged authority, identified as fresh-water species. This seemed to favour the first theory just mentioned. It might, indeed, be asked, Were the data sufficient to warrant the inference? Whatever answer may be given, we are indebted to Mr Brown for the introduction of this new element into these discussions.

1876. Perhaps Mr Brown is seen at his scientific best in the paper "On the Old River Terraces of the Earn and Teith, viewed in Connection with Certain Proofs of the Antiquity of Man," read before the Society in the beginning of 1876, and printed in vol. xxvi. of the *Transactions.* Before noticing the leading characteristics of this paper, I may refer to the circumstances which led to it, and, specially, to the introduction of the speculative element in dealing with Physical Geology phenomena. In 1838 M. Boucher de Perthes, Abbeville, France, published his now well-known book, *De la Création*, in which he expressed the belief that he would find traces of primeval man in the fluviatile gravels of the Somme. In 1846, in another work entitled *De l'Industrie Primitive, ou les Arts et leur Origine*, he intimated that his anticipations had been fulfilled, and in 1847 his *Antiquités Celtic et Antédiluvienne* appeared, giving great prominence to his discoveries in these river gravels. For years little or no interest was taken in his works. But about 1860 the attention of geologists, biologists, and archæologists was fixed on them, and a great controversy arose, in which the giants of the time—Murchison, Lyell, Falconer, Carpenter, and others—were conspicuous. The crucial inquiry came to be, "How was this valley formed?" Lyell thought that "river erosion" will account for most of the phenomena, but added, "I should infer considerable oscillations in the level of the land in that part of France." Murchison took up the same position, but claimed for the phenomena the action of much stronger and intenser forces than Lyell associated with them. In a word, the interest taken in the alleged facts and their discussion was because of the violent contradiction they seemed to give to the prevailing notions as to the time man had been on the earth. That Mr Brown had felt the influence of all this is clear from the summing-up of the results of his observations in the valleys of the Earn, the Teith, and the Spey. As I was myself much interested in the questions raised, I visited the valley of the Somme just when the discussions were at white heat; and when this paper was read, I had an impression that, had Mr Brown spent a few weeks in Abbeville and its neighbourhood, he would not have tried so earnestly to make good an alleged analogy between the formation of our Scottish river valleys and those of England and France. There are proofs of oscillations within the area over which the Somme gravels are spread, to which there is nothing analogous in the gravels of the Earn and the Teith. But all this by the way; and apart from all this, Mr Brown's paper bears in every page the marks of thoroughly scientific work—marks which come out in the careful examination of the valleys, the determination of the relations of the terraces, their levels above the river-beds, and their geological sequence as deposits begun at the close of a glacial period. Then, he argues, came the kames or escars, and last, the collection of the old gravels of which the river floods formed the terraces. Reference is made to the old river terraces of the Spey in support of the Earn and Teith inferences, and, it is asked, how are we to explain the action of the river in throwing up deposits 60 or 80 feet? The answer is, either by floods sufficient to raise the channels to that height, or by supposing the bed of the stream to have been formerly at a higher level than now. Mr Brown pleads in behalf of the former.

The value of these papers on the geology of the surface cannot well be over-estimated. They present, in a most lucid and thoroughly scientific way, questions which still occupy the attention of geologists. If we are ever to have a trustworthy scheme of the order of superposition of quarternary deposits, and a biotic scheme co-ordinate with that of superposition, they are likely to result from such careful observation and orderly records of relation and sequence as distinguish Dr Brown's labours in this department.

II. *Botanical Studies.*—Botany was Dr Brown's earliest and favourite study. Langton and its environments presented a rich gathering ground. The parish lies partly in the Lammermoors and partly in the well-cultivated fields of the Merse. Moor and moss, hill and dale, and the wild-wooded valley through which Langton burn flows, were all that a young enthusiastic botanist could desire. In 1834 he prepared the notice of the botany of the district for the "New Statistical Account." Among the forms mentioned as "lately discovered" is *Saxifraga hirculus*, Dr Johnston's reference to which, in his *Natural History of the Eastern Borders*, is as follows:—"*S. hirculus.* In a wet moorish spot near Langton wood, plentiful, Rev. Thomas Brown, who had the good fortune to add this beautiful species to the Flora of Scotland." When Dr Brown was called to occupy the position of President of the Berwickshire Naturalists' Club, during its jubilee year, 1881, he referred to this in his interesting address. "I remember well," he said, "the enthusiasm with which Dr Johnston welcomed and submitted to the club the little saxifrage from the Langton Lees, and the *Anthoceros punctatus* from the fields of Gavinton—both at the time new to the Flora of Scotland." We have clear proof, in the same address, that, in his botanical studies, he had much more in view than the mere gathering of plants and the attainment of expertness in *hortus siccus* terminology. It was the living form which specially interested him—its relations to other forms, its surroundings, its use, the use of its beauty, and many such-like elements associated with place, and habits, and appearance and structure. The numerous references to him in Dr Johnston's work show how thoroughly he had mastered the botany of his native district.

III. *Literature.* — Dr Brown's work in this department was mainly — (*a*) Biographical, and (*b*) Historical. Or, perhaps, it would be better to say that it was history from the biographical point of view. In a general way, this may be affirmed of both of his works—*Annals of the Disruption*, and *Church and State in Scotland.*

But, as the subjects dealt with in them lie outside of those chiefly dealt with in this Society, I do little more than name them, in order that the record of Dr Brown's work, all round, may be as full as possible. In the *Annals* he sought to do for the Disruption men something analogous to what Dr Calamy did for the 17th century Nonconformists in his *Nonconformist's Memorial*, but, both in subject-matter, in style, and in the gift of supplying a setting for dry ecclesiastical details and incidents by associating them with phases of social, domestic, or religious life, which are ever fresh and interesting, the *Annals* take the foremost place. The goodly volume on *Church and State in Scotland* consists of six lectures delivered by Dr Brown as "Chalmers Lecturer." I may state that, in 1880, Robert Macfie, Esq., of Airds and Oban, transferred £5000 to trustees for the founding of this lectureship in memory of Dr Chalmers, and in connection with the Free Church. I heard all Dr Brown's lectures, and was struck with their clear, crisp style, graphic descriptions, and wide-minded appreciation of praiseworthy points, irrespective altogether of party considerations.

In the foregoing notes little has been said of his work as a Christian minister, though it was in this that his best qualities found highest expression. His friends love to say that, had he devoted his time to scientific pursuits, he might have taken a distinguished place among men of science. But the fact that all the points of his individuality fell so well into the profession of his choice makes this doubtful. It was in fulfilling the life-work to which he was set apart, that his quiet gentlemanly bearing, cultivated mien, extensive yet accurate knowledge of books and of men, his ever thoughtful consideration for the opinions of those from whom he differed, and his ready though never obtrusive exercise of the charity that suffereth long and is kind, were signally manifested. Unlike so many of his class, he had qualified himself to read both branches of the one revelation of God to man, and had found in Nature not only a revelation but a mental discipline also: "Homo, naturæ minister et interpres, tantum facit et intelligit, quantum de naturæ ordine re vel mente observaverit; nec amplius scit aut potest."—Bacon, *Nov. Org.*, Aph. i.

## Donald Beith. By Patrick Murray, Esq., W.S.

(Read December 17, 1894.)

Donald Beith was the son of Mr Gilbert Beith, farmer, Lochgilphead, and was born there on the 25th of November 1815. When he was quite a small boy, his father, who was a highly educated man, detected in a strolling player, called Dunlop, who came to Lochgilphead, an excellent classical scholar. He learned that he was the son of a clergyman in the north of Ireland, and a graduate of Dublin University. The old man took a great interest in him, and urged him to give up his wild, wandering life, promising that if he stayed in Lochgilphead and opened a school, he should have Donald for his first pupil. This was done with very happy results, and Donald and his teacher became devoted friends, and in the later years of Dunlop's life, when things were low with him, Donald Beith was his chief support, and no one knows how much he did for his old teacher. After being educated in Lochgilphead by Dunlop, he served in a legal office in Campbeltown for some years, and then came to Edinburgh, and was indentured to the law under Messrs James Greig & Charles Morton, W.S., Edinburgh. When he left that firm's employment he went for some time into the office of Messrs Gibson-Craig, Dalziel, & Brodie, W.S., and, about the year 1848, he entered into partnership with Mr Andrew Murray, W.S., under the firm of Murray & Beith. In the year 1850 he was admitted a member of the Society of Solicitors before the Supreme Courts of Scotland, and in 1862 of the Society of Writers to the Signet. Upon the death of Mr Andrew Murray in 1869, Mr Beith was appointed to succeed him in the office of agent for the Woods and Forests in Scotland, an office which he held till his death, and he was also agent for a number of other Government departments in Scotland, including the Treasury, the War Department, the Harbour Department of the Board of Trade, the Board of Works, the Education Department, the Prison Commissioners for Scotland, &c. His business was, after the death of Mr

Murray, carried on by himself and the other partners of the firm of Murray, Beith, & Murray, W.S. In connection with the inquiries he had to make for the different Government departments for which he acted, Mr Beith acquired a wide knowledge of all subjects affecting antiquarian legal matters in Scotland, and was especially versed in the law of Teinds, Salmon-fishings, and the different tenures of land in Scotland. He always took a keen interest in politics, although latterly his official connection with Government debarred him from active participation in political affairs. In the earlier years of his professional career he was a liberal of an advanced type, and did good work for his party when, as agent for Mr Charles Cowan, he was helpful in defeating Macaulay as the representative of the city of Edinburgh. At a later date he acted as agent for Sir Alexander Gibson Maitland, when the latter defeated Lord Dalkeith in a contest for the seat for the County of Midlothian. Though holding many progressive liberal views, Mr Beith had little sympathy with the Home Rule movement, and stood firm with the Liberal Unionists for the maintenance of the Union. A staunch Free Churchman, he was an elder in Free St George's Church, and no member or office-bearer in that communion was more thorough and devoted in the practice of his principles.

His strength of character was great, and it was founded on a strong and simple belief in the truths of the Bible. He judged himself severely, but no man was more lenient in his judgment of others. Charitable to a degree, he was ever ready to lend a helping hand in the furtherance of philanthropic or religious schemes, more especially those connected with the Free Church, but all his kindness and charity were of the most unostentatious nature.

He had a great charm of manner and power of attracting others, and the affection which he bestowed on his friends bore a fruitful harvest in the numbers who really mourned his death. He delighted in hospitality, and in having his friends about him at home. His zeal for his clients was remarkable, and he was untiring in the work which he did for them. His whole interests were centred in his business and his clients, and he rarely took a holiday or spared himself in any way. On the last day (at the age of nearly 79) on which he attended his office, he wrote letters with his own hand, and then, without telling any one, walked home to undergo the operation from the effects of which he succumbed ten days later. He died upon the 9th of October 1894, and he may literally be said to have died in harness,—a good man, whose chief characteristics were strong faith in the Christian verities, great simplicity of character and unselfishness, extreme warm-heartedness and charity, indomitable pluck, and an undeviating devotion to whatever he conceived to be his duty.

In 1870 he married the widow of his late partner, Mr Murray, and is survived by her. He left no family.

## William Durham. By Professor C. G. Knott, D.Sc.

(Read January 21, 1895.)

William Durham was born at Edinburgh in November 1834. He received his education at the Edinburgh High School; but, much to his own disappointment and that of the rector, was removed at a comparatively early age and put to business. After spending some years in the publishing-house of Adam Black, senior, he was taken into his father's business of wholesale stationer and paper-maker. But Mr Durham was, by nature, a student. Science, especially chemistry, was his chief interest through life. It was this interest which sustained him in spirit amid the trials and disappointments that are almost inevitable when a man is launched on a career essentially out of harmony with his whole bent of mind. When a little over twenty years of age, William Durham set up his private laboratory at Glenesk House, Loanhead; and experimental work occupied much of his attention to the day of his death, January 23, 1893.

He was one of Professor Tait's earliest laboratory students, and his first paper communicated to this Society is a record of work done there. The title is "On the Currents produced by Contact of Wires of the same Metal at different Temperatures," read 3rd June 1872 (*Proc. Roy. Soc. Edin.*, vii. pp. 788–791). The investigation was undertaken at Professor Tait's suggestion, and the general result is that up to temperatures of 325° C. the transient current obtained by contact of hot and cold platinum wires is proportional to the difference of temperature. The novelty of the method employed deserves mention; and also the peculiar difficulty of getting exactly similar contacts between exactly similar pairs of surfaces at given temperatures. The constancy in the results obtained by Mr Durham attests his skill and patience as an experimenter.

Mr Durham was elected a Fellow of the Society in February 1874, and all his other papers have to do with solutions. The most important of these, read January 21, 1878, is on "Suspension, Solution, and Chemical Combination" (*Proc. Roy. Soc. Edin.*, ix. pp. 537–541). His main conclusions are, that between suspension and chemical combination there is no break in the series of grades of solution; that chemical combination, solution, and suspension differ only in degree; and that the attraction of chemical affinity is not, in all cases at any rate, exhausted when a definite compound is formed, but has sufficient power left to form solution or suspension compounds. His latest paper, on the "Laws of Solution" (*Proc. Roy. Soc. Edin.*, xiv. pp. 381–387), discusses the last conclusion from the point of heats of combination. The first and second conclusions form the main theme of a very recent paper on "Solution and Pseudo-Solution" (*Trans. Lond. Chem. Soc.*, 1892), by Messrs Picton and Linder, who seem to have been unacquainted with Mr Durham's pioneer work. Other papers by Mr Durham on the same subject will be found in vols. xi., xiii., and xiv. of the Society's *Proceedings*; also in the *Chemical News* (1878), in *Brit. Assoc. Reports* (1887), and in *Nature* (vol. xxxvi.). Working at a time when the modern electrolytic theory of solution was but dimly apprehended, Mr Durham was unable to develop his views to their full significance. We should, perhaps, regard them as a first statement of an important aspect of the modern theory. In regard to Mr Durham's powers as a practical chemist, Professor Crum Brown writes:— "When I was asked to report on the means of purifying the Gala Water, I selected Mr Durham as assistant in the practical work and analysis, my choice being very much determined by the character of the papers which he had communicated to the Society. In that and in other similar work in which I had the advantage of his assistance, I found his uniform care and accuracy of great service."

Mr Durham was the writer of the long series of scientific articles which appeared with fair regularity, week by week, in the *Scotsman* newspaper from October 4, 1886, to December 3, 1892. The more important of these were republished in book form under the general title of *Science in Plain Language* (A. & C. Black, 1889–91). The first volume treats of "Evolution, Antiquity of Man, Bacteria," &c.; the second of "Astronomy — Sun, Moon, Stars," &c.; and the third of "Food, Physiology," &c. These titles sufficiently indicate the wide range of subjects handled by him. The style is easy and clear, the exposition thoroughly scientific and up to date. Quiet and retiring in disposition, and almost diffident in manner, Mr Durham was a man whose native ability was apt to pass unrecognised by the ordinary eye. But in these articles, so admirable in their accuracy and in their freedom from all false rhetoric, we have a lasting revelation of the truth-loving, unostentatious character of him who wrote them.

## Alexander Leslie, F.R.S.E. By James Brand, Esq.

(Read January 21, 1895.)

Alexander Leslie, born at Dundee on 16th September 1844, was a son of the late James Leslie, M.I.C.E., who died just five years ago. Mr Leslie was educated at the Edinburgh Academy and the Edinburgh University, and in May 1862 entered the office of Messrs D. & T. Stevenson, M.I.C.E., the lighthouse engineers.

After serving an apprenticeship of three years with Messrs Stevenson, during which he was on the Wick Breakwater Works, he was engaged for some time with Mr MacBey, land surveyor, Elgin, so as to acquire some experience in land surveying, which could not be had in the south to the same extent, owing to the country having all been mapped out by the Ordnance Survey.

He was afterwards engaged in the north of England on the staff of Messrs Morkle & Prodham, contractors for the Blaydon and Conside Railway. He thereafter entered his father's office as an assistant in the year 1865, and in 1871 was assumed as partner by his father, the name of the firm being Messrs J. & A. Leslie.

Mr Leslie took an active share in the business of the firm, which has been extensive and varied. He was especially engaged in the construction of the Edinburgh Waterworks (Moorfoot Extension), Dundee Waterworks (Lintrathen Extension), also waterworks for Berwick-on-Tweed, Peebles, Dunbar, Peterhead, Thurso, Kirkwall, Lerwick, Galashiels, Bothwell, St Andrews, Leven, and many others of less magnitude. He also carried out harbour works at Montrose, and drainage works at Kirkwall, Lerwick, &c., and was consulted as to many other waterworks, among which may be mentioned Dumfries, Perth, Aberdeen, and Swansea. He acted as Valuator for the Board of Trade in Scotland under the Railways Clauses Act, and was largely engaged in valuations and arbitrations, and in giving evidence before the Courts on engineering questions.

He was frequently employed in Parliament in supporting schemes for which his firm were the engineers, and in supporting or opposing the schemes of other engineers. He made an excellent witness, and took care to be always well prepared, and was very ready in picking up any flaw in his opponent's case that might emerge in the course of the inquiry.

He had special experience in the construction of reservoir embankments, and had a remarkably quick eye in detecting faults of construction in any piece of work. He had a high ideal of what work ought to be, and it was always his aim, by careful preparation of specifications and close supervision during execution, to attain perfection as far as possible. It may safely be said that the works that have been carried out under his charge bear witness of the thoroughness of his supervision. Since his father gave up active work in 1880 he took chief charge of the maintenance of the Edinburgh Waterworks, which are varied and extensive, there being no less than twelve reservoirs, some of great magnitude, and necessarily include a great length of piping over an extensive area of distribution.

That his services were highly appreciated will be gathered from the minute of the meeting on 14th December of the Edinburgh Water Trustees, of which the following is an excerpt :—

> "Prior to considering the business before the meeting, the Convener referred to the loss the Trust has sustained by the death, on the 7th instant, of Mr Alexander Leslie, C.E., senior partner of the firm of Messrs J. & A. Leslie & Reid, the Trustees' Engineers. During his long connection with the Trust, Mr Leslie, he said, had been distinguished by the zeal and ability which he displayed in regard to its affairs, and by his thorough independence and honesty of purpose, and the faithful manner in which he discharged his duty to the Trustees and to the public."

He was a Fellow of the Royal Society of Edinburgh, and of the Geological Society of London, and was President for two years of the Royal Scottish Society of Arts, and only demitted office a few weeks before his death, when he gave a very interesting address on the more modern system adopted for the examination of water, making special reference to the bacteriological aspect of the question. Expression was given to the feelings of that Society by his successor in the Chair in the following terms :—

> "At the meeting of the Royal Scottish Society of Arts last night, the President (Dr William Taylor) moved that the meeting be adjourned out of

respect to the memory of Mr Alexander Leslie, their late distinguished President; and that they should record in the minutes an expression of their deep sense of the loss they had sustained by his death, and of their sincere sympathy with Mrs Leslie and her family circle in their sudden and severe bereavement. That day many of them had followed to the grave all that was mortal of their late beloved President. Few could realise how much he would be missed, for they all knew that no one loved the Society more than he, or contributed more towards the elucidation of the varied subjects which came before it. His mental grasp seemed to be all-embracing, and he seemed to have the power of mastering the intricacies of a new problem almost before its demonstration was completed. He worked silently and unostentatiously amongst them, but his influence was widely felt, and obtained for them the addition to their roll of many illustrious names. His sudden and early death has stirred every emotion of sympathy within them, and left a blank which they could not hope to fill. He dared not speak of what Mr Leslie was to himself personally, and to all of them who knew him as a friend. He dared not speak of the many evidences of the ready hand and the generous heart which were indirectly known to those who experienced them. He was following the dictates of his own heart, and giving expression to the feelings of all, when he proposed that they should adjourn. Dr R. M. Ferguson seconded the motion, which was agreed to unanimously."

Mr Leslie contributed several papers to the *Proceedings of the Institution of Civil Engineers*, among which are accounts of the Paisley Waterworks and Edinburgh Waterworks, and a description of the various kinds of salmon-ladders in use in Scotland.

He also contributed to the Royal Scottish Society of Arts the following papers :—Rainfall and Evaporation, an account of Berwick Waterworks; Description of an Improved Joint for Levelling Staff; an account of the Dundee Waterworks; and Notes on Experiments of the Flow of Water over Triangular Weirs; for each of which he received the Society's silver medal.

Mr Leslie had a highly versatile mind, and was possessed of many accomplishments. He had travelled much, and his great sense of humour, coupled with his great powers of observation, made him an excellent travelling companion, and brought him a large circle of friends.

His humour was of the old Scottish quality, quaint and pawky, and evidently inherited from both his parents, as all who enjoyed the pleasure of his father's acquaintance and friendship, or of his uncle's, on the mother's side, the late John Hunter, W.S., of Craigcrook, could easily perceive.

Mr Leslie acted in 1893 as a witness for the Caledonian Railway

Company, in the case of Mrs Armour against them, which was an action of damages arising out of the subsidence of a trench formed for the construction of a sewer in Stevenston Street, Glasgow. The water-run of the sewer was about 30 feet from the surface of the street, and the trench was about 8 feet from the building-line. The method adopted in constructing the sewer was open casting by means of hand-piling, and during a heavy rainfall part of the open trench gave way, and serious injury was done to the adjoining buildings.

Mr Leslie, in the circumstances, was called upon to defend the method of hand-piling adopted. There were other three methods suggested, viz., ordinary tunnelling, tunnelling by compressed air, and steam-driven piling. Mr Leslie had great experience of works of this nature, his firm having carried through many important contracts—notably, a portion of the Leith Purification Scheme, which passed through some of the most important streets in Leith; and in preparation of Armour's case he made many helpful suggestions, not only to those entrusted with the conduct of the case, but also to his co-witnesses. It was evident that he had profited by his experience, and one felt that when he spoke he did so as one having authority. His appearance in the witness-box was quiet, dignified, and impressive, his evidence being given in a clear, precise, emphatic manner, coupled with that moderation and fairness which are always so telling in their effect on those sitting in judgment on the case.

Mr Leslie also acted as a valuator appointed by the Board of Trade in a question arising out of the construction of the Tollcross Lines, and in that matter his actions were characterised by promptness and despatch. He showed that he was a man thoroughly capable of discharging judicial functions, and his judgment was by the claimant and by the railway company regarded as being fair and reasonable.

Mr Leslie was of active habits, and was able to attend to business to within a week of his death, which came at the last with startling suddenness. He was cut off at the age of 49, and leaves a widow and son to mourn his untimely death.

## General Robert Maclagan, R.E. By Major-General Sir Robert Murdoch Smith, K.C.M.G., R.E.

(Read January 21, 1895.)

General Robert Maclagan of the Royal (late Bengal) Engineers, who was born in Edinburgh on the 14th December 1820, was the third son of Dr David Maclagan, Physician to the Forces and Surgeon-in-Ordinary to the Queen in Scotland. He was educated at the High School and at the University of his native city. Subsequently, after the usual course of instruction at the East India Company's Military College at Addiscombe, where he greatly distinguished himself, he received his first commission as a Second Lieutenant in the Bengal Engineers in December 1839. Like other young Engineer officers, he thereupon underwent a thorough course of practical training at the Royal Engineer Establishment, Chatham, now designated the School of Military Engineering, before proceeding to India to enter on the active duties of his profession. It may not be amiss to point out that, in all probability, much of the thoroughness and versatility which characterised his subsequent career was due to the nature of his early training, viz., a good school and university curriculum under home influences, followed by two years' theoretical, and two years' practical, instruction in the special subjects appertaining to the duties of a military engineer. This happy sequence of literary, scientific, and practical pursuits undoubtedly helped, in large measure, to prepare him for the varied eventualities of his future career.

Arriving in India, he was appointed to the Bengal Sappers and Miners, the headquarters of which corps he joined at Delhi in March 1842, while the British and Indian forces were still engaged in avenging our previous disasters in Afghanistan. From Delhi he marched in the same year in command of a company to Firozpur, where he joined the army of reserve held in readiness to support the army in the field under Sir Frederick Pollock. On the safe return of the latter, after its victorious campaign, the army of reserve was broken up, and Maclagan was transferred first to Kurnal, and afterwards to Karachi, the port of the recently annexed province of Sind. There, under the energetic Sir Charles Napier, he acted for a short time as Executive Engineer until the outbreak of the Sikh war in December 1845, when, along with his chief, he started for the Punjab. He arrived at Lahore in time to take part in the grand review of the British forces on March 5, 1846, by the Commander-in-Chief, Sir Hugh Gough, and the Governor-General, Sir Henry Hardinge. In the course of the war he was placed in charge of the works for the defence of Lahore, a most responsible post for so young an officer. The choice of Maclagan for such a post is, in itself, ample evidence of the fact that at this early period of his career he had already gained the confidence of his superior officers. After a few months' service at Lahore, which, contrary to expectation, was not attacked by the Sikh army, he was prostrated by fever, and sent to Simla. While there he was in the following year selected for the position with which his name was afterwards to be so thoroughly identified, that, namely, of Principal of the Civil Engineering College about to be established at Rurki.

The idea of training young Europeans, Eurasians, and natives in different branches of civil engineering, so as to fit them for useful employment in the Public Works Department, was a new one, and the means adopted for carrying it into effect were, consequently, experimental. On the selection of the first Principal and Organiser of the new College depended the success or failure of the experiment. That, under such circumstances, the Government should have chosen a subaltern little more than twenty-six years of age, shows clearly how young Maclagan's capacity, character, and personality had impressed themselves on his contemporaries.

The result showed that the confidence of the Indian Government in the young Principal was not misplaced. From the first he displayed an extraordinary talent for organisation, and an indefatigable habit of taking pains. These qualities, combined with a remarkably genial and kindly disposition, made his reign at Rurki an eminent success.

When the Mutiny broke out in 1857, measures, in which Maclagan took a prominent part, were at once taken by Colonel Baird

Smith, R.E., for the defence of the European community in and around Rurki. The college buildings were put into a state of defence, and the pupils organised into such an efficient garrison, that the mutineers thought it prudent to let them alone. What might otherwise have proved a second Cawnpore was thus happily averted. Throughout that trying and critical period Maclagan's conduct was spoken of by those who were present as beyond all praise.

In 1861 Maclagan, now a Lieutenant-Colonel, was appointed Chief Engineer and Secretary in the Public Works Department to the Government of the Punjab, in which appointment he remained until his retirement from the service in January 1879, after attaining the rank of General. During his long administration of the Public Works Department in the Punjab, much was done in the making of roads, railways, and canals, and in the erection of barracks and other public buildings, towards the development of the resources and the permanent security of what in many respects is the most important province of the Indian Empire.

In retiring from the public service, General Maclagan merely exchanged one field of activity for another. In the *Proceedings of the Royal Asiatic Society*, of which he was a Member of Council, and in the latest edition of the *Encyclopædia Britannica*, he found scope for his literary tastes and researches, more especially in subjects connected with the East; while his continued interest in science was evidenced by his Fellowship of the Royal Society of Edinburgh, and of the Royal Geographical Society of London, of which latter, for a number of years, he was a most efficient Member of Council. It was, however, on work of a missionary and philanthropic nature that his heart was chiefly set. On it he spared neither time, money, nor labour, and whatever his hand found to do in its furtherance he did it with his might.

In every relation of life, public and private, he exemplified, as few men have done, the apostolic definition of charity, which reads almost like a categorical description of the character, temper, and disposition of Robert Maclagan.

## Dr Sanderson. By Dr Buchan.

(Read February 4, 1895.)

Dr James Sanderson was born at Dunbar on May 21, 1812, and died March 28, 1891. He was educated at the Grammar School there, and thereafter entered the University of Edinburgh as a medical student.

After graduation, his first appointment was that of Surgeon on board the East India Company's ships "Marquis of Camden" and "Duke of Argyll," on voyages to St Helena, Bombay, China, Calcutta, and Ceylon. In 1836 he was appointed Surgeon in the Madras Medical Service, and in 1837 did duty with the artillery corps at St Thomas's Mount.

He was appointed in 1838 by Lord Elphinstone, then Governor of Madras, to organise the medical department in connection with the system of convict labour instituted by his Lordship. For the successful accomplishment of this Dr Sanderson received the thanks of the Government. He was next appointed one of the medical officers of the Neilgherries, which post he occupied for the next three years.

He was placed on the Presidency Medical Staff in 1844 as Port and Marine Surgeon, and afterwards was appointed District Surgeon by the Marquis of Tweeddale, at that time Governor and Commander-in-Chief of Madras. Shortly thereafter he became medical attendant to his Lordship and suite, and subsequently served in the same capacity to Sir Henry Pottinger, who succeeded Lord Tweeddale as Governor of Madras; and also to Sir George Berkeley and General Strachey, Commander-in-Chief of the Madras Army. In 1854 he was appointed Garrison Surgeon of Fort St George, Madras, and accompanied Lord Harris as medical attendant in his several tours through the provinces, and returned to England with his Lordship in 1859.

In the following year he returned to Madras, was appointed to the Governor's Body Guard, was sent to Galle to meet Sir

William Dennison, whom he accompanied on his tours throughout the Presidency in 1861–62. He acted as medical attendant to His Excellency, and also to his successor, Sir Hope Grant, and their suites. In May 1863 he was placed on the retired India list.

From that time he resided in Edinburgh, and took an earnest and active part in professional, scientific, philanthropic, and religious movements. He was elected a Fellow of this Society in 1863, and for some time was a Member of Council. In the same year he became a Member of the Scottish Meteorological Society, and a Member of its Council in 1865, Honorary Treasurer in 1872, and Honorary Treasurer to the Ben Nevis Observatory in 1883, when the Observatory was established. He was most regular in attendance at Council, Committee, and General Meetings, and devoted a very large portion of his time not merely to the more special duties of Treasurer, but also in forwarding the extension of its membership, and in promoting whatever tended to increase the efficiency of the work of the Society.

From the beginning of his career to the end, Dr Sanderson was an ardent learner. After settling in Edinburgh, he attended several of the medical classes of the University with the view of being brought more abreast with the different departments of his profession; and latterly, when Dr Whyte began his classes for young men on Sunday evenings, which are in no small degree academical in character, he, and his friend, Mr Donald Beith, whose obituary was read at last meeting, were among the most regular attenders of the class.

His professional success in India was the result of the remarkable openness and teachableness of his mind in quest of information from all quarters, his firmness of character when required, but, above all, to the unfailing cheeriness of the man, which inspired hope and ultimate recovery to many a sick-bed. In these various capacities he was able to perform most effective service by his earnestness of purpose, his enthusiastic nature, and his singularly genial and kindly manner.

## Dr Hugh Francis Clarke Cleghorn. By Professor M'Intosh, St Andrews.

(Read July 1, 1895.)

Dr Cleghorn was descended from an old Fife family, his grandfather having been Professor of Civil and Natural History in the University of St Andrews, and was born in Madras on the 9th August 1820, his father being then Administrator-General in the Supreme Court. He was sent home in 1824, and resided at the family estate of Stravithie, near St Andrews, till he was twelve years of age. As a boy he was trained to rural pursuits, and rendered familiar with agricultural routine. These early lessons amidst the fine woods of Stravithie, then haunted by the roe-deer and wild-duck, seemed to have laid the foundation for the love of flowers, shrubs, and trees that in after-life became so pronounced. He was sent to the High School of Edinburgh, which he attended for two years—having for school-fellows the brothers Philip and Robert Maclagan, William Nelson (afterwards the publisher), and the Rev. Prof. Milligan, late of Aberdeen.

Leaving the High School, he entered the University of St Andrews, where, besides the ordinary classes, he had the privilege of attending a short course of lectures by Edward Forbes on star-fishes, before the publication of his classic work on that subject. After studying in the Arts classes for four years, he was apprenticed in 1837 as a pupil of the distinguished Edinburgh surgeon, Professor Syme, for five years, holding, however, during the fifth year, the office of House-Surgeon in the Edinburgh Infirmary. During his career in Edinburgh, botany, then under the charge of Prof. Graham, formed a favourite study, and laid a firm hold on the young surgeon—one of the numerous instances of the brotherhood that from earliest times has always subsisted between medicine and biology.

Having graduated at the University of Edinburgh in 1841, and obtained an appointment in India in 1842, he proceeded, at the age of twenty-two, by a sailing-ship to Madras—the voyage then occupying three months. Landing in December, he was attached to the Madras General Hospital, to study Indian diseases, and thereafter, for three or four years, led a life of constant marching and counter-marching with different regiments. He thus obtained many opportunities both of learning the native language and of extending his botanical knowledge, which, originally fostered by a diligent use of the Edinburgh Botanic Garden, now began to bear fruit. His other duties consisted of the superintendence of a jail and the practice of vaccination, besides preparing a collection of native raw produce for the local museum. Following out a suggestion which he received from Sir William Hooker, he was in the habit of studying a few plants daily, and thus acquired an extensive knowledge of the medical and economic plants of India. He was further encouraged by letters from Sir Robert Christison, with notes of *inquirenda* and *desiderata* relating to Indian drugs, such as gamboge and chiretta.

His botanical tendencies, indeed, even then attracted considerable notice, especially his observations on the destruction of the forests, and he was appointed on the Mysore Commission, chiefly in connection with this subject. The labours entailed on Dr Cleghorn at this time, however, told on his health, and, early in 1848, sick of Mysore fever, he was sent home. The voyage proved disastrous, for the ship was totally dismasted, lost five of her able-bodied seamen, and the passengers were with difficulty landed—without either luggage or money—at Cape Town. He reached England at the end of June, but was still in weak health, for an attack of pleurisy with cough had followed the Mysore fever. He recruited by botanising in Devonshire; and thereafter, having attended the meeting of the British Association in Edinburgh in 1850, he was appointed, with other eminent men, to report upon tropical forests, and the influence which they exerted on the climate and the resources of the country. Dr Cleghorn, who drew up this exhaustive report, had, indeed, early perceived the immense importance of the tropical forests. He had observed that, as the population spread out, the people were tempted to invade the forests and cultivate within them. More than thirty years ago, so impressed was he by the results of what was known as the "Kumri" cultivation, that he was instrumental in getting orders issued by the Government to stop the wasteful system in Mysore. As Sir Dietrich Brandis and Sir William Muir so clearly pointed out* some years ago, he was just the man who could best carry out forestry measures amongst the people of India—without appearing tyrannical. "He was," says his colleague, Sir Dietrich Brandis, "known to be a true friend to the natives, and had made himself familiar with their modes of life and systems of husbandry. As a medical man his name was also widely known, and he had acquired much influence amongst the native population; and, indeed, Dr Cleghorn's single-minded desire to promote the welfare of the people had become evident, not only to the natives, but also to leading Government officials in Madras, and the confidence they placed in him was the secret of his subsequent success in this important matter." †

About this time Dr Cleghorn contemplated retiring from the service, and, indeed, his papers were drawn up, when he met Prof. Forbes Royle, of King's College, London, who asked and obtained his aid in preparing a Catalogue of the Raw Products in the great Exhibition of 1851, a task which occupied him ninety days. Forbes Royle was very thankful for the valuable help given him by the young Indian surgeon, for his own health had become somewhat feeble. He, moreover, gave him letters to the India Office, to Sir Henry Pottinger, Governor of Madras, and to others. This, and an improvement in health, led to Dr Cleghorn's return to India in 1851, and he lost no time in calling on the Governor of Madras, who took a deep interest in him. He was then given orders to proceed with a wing of a Queen's regiment to Trichinopoly; but he had only been there three weeks when he was recalled, and offered the post of Professor of Botany and Materia Medica in Madras. Shortly after settling in this congenial office, he was also put in charge of a number of young forest-officers, and no one with greater aptitude could have been selected, for he was not only familiar with Indian life and its dangers, but had always been characterised by his kindly interest in young men, and still more by his high moral tone and his strictly temperate habits. Thus his income was increased and his botanical tastes were given free play.

* Royal Scottish Arboricultural Society, 7th Aug. 1888; *Trans.*, ibid., xii. pp. 87–93.

† Sir Dietrich Brandis, *Trans. Scot. Arboric. Soc.*, xii. p. 90. A generous tribute to his fellow-labourer, Dr Cleghorn.

For some years he laboured in Madras in these offices, and also acquired a fair practice in the city, in addition to carrying on the duties of Port and Marine Surgeon, and afterwards of District Surgeon of St Thomé. But this quiet life was by-and-by broken. In 1855 he received an invitation to Government House, and found that, besides Lord Harris, then Governor of Madras, the only other person present was Sir Arthur Cotton. Before leaving, Lord Harris asked him to look at a bundle of papers connected with public works, especially railways, and explained that one of their difficulties was to get wood for sleepers, adding that Sir Walter Elliot had said he was the best man to consult. After some further conversation about a forest department, and the changes that would be necessary in Dr Cleghorn's official position, the latter left, saying that he would think over the matter for a week, as he was doubtful where he could get assistance. He consulted Sir Walter Elliot, a man not only of great experience and sagacity, but well-known for his love of science, and at the end of the week he returned to Lord Harris, and accepted the task of organising a forest department, and reporting. He was then transferred from the military to the revenue department.

He first visited Burmah, and saw the working of the trained elephants; next, Dr Thomson of the Calcutta Botanic Garden; and obtained from every available source, and at considerable personal exertion, the necessary information. At the end of two months he presented his preliminary report to the Governor, embodying the scheme for a forest department, afterwards to become so important in India. Sir Dietrich Brandis was appointed by Lord Dalhousie, the Governor-General, to the newly-acquired province of Pegu, as Conservator of Forests in Burmah, while Dr Cleghorn worked on in Madras, and they frequently consulted together, so as to act on the same lines.

At this time (1861) Dr Cleghorn visited his home in Scotland, taking a brief holiday of a few months, and marrying Miss Cowan, daughter of Mr Charles Cowan, late M.P. for Edinburgh. Returning to India, he landed with his wife at Galle in Ceylon, where he received a telegram from the Government requesting him to return to Simla. On arrival he was appointed Joint-Commissioner with Sir D. Brandis for the conservancy of forests. The Governor-

General further desired Dr Cleghorn to proceed to the Punjab to examine the forests of Western Himalaya, and to institute a systematic plan of conservancy and management. He spent three years in exploring the countries adjacent to our north-west frontier, including part of Kashmir and the Trans-Indus territory. The fine series of photographs taken during his journey to Kashmir give a vivid idea of the remarkable geological formation, of the richness of many parts in pines and other trees, and of the general configuration of the region.

The two commissioners met frequently, and finally presented reports. Dr Cleghorn's was published in 1864, and forms a large octavo volume, with various maps and plans, exhaustively dealing with the forests of the Punjab, and in a manner that reflected the highest credit on his ability as a scientific botanist and experienced administrator in forestry.

Having thus introduced the system of forest conservancy in the Punjab, Dr Cleghorn returned to Madras, and carried on the duties of Conservator of the Forests in that Province. "He had the satisfaction of accomplishing for the Madras Presidency the same result, which thirteen years previously he had helped to bring about in Mysore. The Government prohibited Kumri cultivation in the forests without previous permission."* Meanwhile, Sir D. Brandis, whose experiences of forests were European, for he had studied the subject in Germany, Saxony, and France, as well as in India, was appointed the first Inspector-General of Forests to the Government.

Life passed pleasantly in Madras for some years, but his father dying at Stravithie in 1864, he came home on short leave in 1865. He also acted as Inspector-General of Forests when Dr Brandis visited this country in 1867. Finally, he retired from Indian service in 1869.

Besides his official reports, he published a work in 1861 on the "Forests and Gardens of South India"—in which the conservancy of the forests, the economical method of supplying fuel, and the modes of treating the several trees, as well as the condition of the various gardens, were fully illustrated. As Principal Sir William Muir stated some years ago,† this work was extremely useful in spreading

* Sir D. Brandis, *op. cit.*, p. 90.

† *Trans. Roy. Arbor. Soc.*, xii. p. 200.

a knowledge of the value of forests, and the best means of assisting their cultivation. His Indian experiences were also given in many separate papers published between 1850 and 1870, such as those on the "Hedge-plants of India," "Sand-binding Plants," "Chiretta used in the Hospitals of Southern India," "The Indian Gutta Tree," "The Coco-nut Tree," "The Varieties of the Mango Fruit," "The Introduction of Cinchona Trees," and numerous others dealing with trees, general vegetation of districts, and accounts of his expeditions,—one of the latter, viz. that to the higher ranges of the Anamalai Hills, having been published in the Society's *Transactions*. Thus, whether we regard Dr Cleghorn's great services to the cause of forestry in India, his labours in general botany, or his official duties in connection with the army, it is seen that the Minute of the Government on his retirement was fully merited. It runs as follows:—"His long services, from the first organisation of forest management in Madras, have, without question, greatly conduced to the public good in this branch of administration; and in the Punjab also Dr Cleghorn's labours have prepared the way for the establishment of an efficient system of conservancy and working the forests of that province."

In the summer of 1869 he was suddenly called, by the serious illness of Prof. Walker Arnott, to undertake the duties of his class in the University of Glasgow. He then gave a complete course of lectures on Systematic Botany, besides conducting the various excursions of the class.

Dr Cleghorn now took up his residence at his estate of Stravithie, and entered keenly into every philanthropic movement in the county. As justice of the peace, and member of most of the important county committees, his time was fully occupied. His efforts to promote the cause of temperance also deserve grateful remembrance. As prison visitor, by his kindly sympathy and advice, he obtained a hold on many an unfortunate criminal, whom, on discharge, he was the first to assist pecuniarily, as well as to aid in obtaining employment.

While thus largely occupied with philanthropic and county business, he still found time to pursue his favourite studies of botany, and especially of forestry. In 1870, ten students of forestry, who had been driven from the banks of the Rhine by the contending armies, were placed under his care. He continued their studies by a course of twenty lectures on forestry in the United College, St Andrews, which were attended also by the general students of the University, attracted no less by the interesting nature of the subject than by the great experience and the sympathetic bearing of their teacher. The grounds of Stravithie were studded with rare pines and shrubs, and he was ever ready to invite and welcome botanical and other visitors to see them. Nature had been lavish in the botanical treasures of his neighbourhood, and by judicious planting, art greatly increased its amenity. He was one of the leading members, and President in 1870, of the Botanical Society of Edinburgh, his presidential address being devoted to a review of the advances in botany since he joined the Society in 1839, and a summary of the present state of the science in Scotland. The congenial presence of old friends like Sir Douglas Maclagan, the late Professors J. H. Balfour, Sir Robert Christison, and others, at this Society, and at the Royal Scottish Arboricultural Society, was to him a constant source of pleasure. He was indefatigable in advancing the interests of the latter Society, and he was twice President. For many years he selected candidates for the Indian Forest-Service, and it was his evidence before the Forestry Committee of the House of Commons that was mainly instrumental in the formation of a Forest Board to promote the proper training of young men for forest-service, and which also led in part to the establishment of a Forest-Branch in the Engineering College at Cooper's Hill, Surrey. On retiring from the India Office, he received a complimentary acknowledgment of his services from the Secretary of State, mentioning, amongst other things, that all the present forestry-officers in India had passed through his hands. For ten years he acted as Examiner in Botany in the University of Edinburgh, and no one more assiduously or more conscientiously performed his duties. For twenty years he was also Examiner in Forestry to the Highland and Agricultural Society.

No one was more active or more persevering in furthering the project of an International Forestry Exhibition when it was mooted in Edinburgh in 1883, and his extensive acquaintance with those most likely to aid on the Continent, in America, and in the Colonies, was of signal service to the executive. The success of this important

Exhibition, indeed, was largely due to his unceasing efforts; and further, he roused public attention to the need of more complete and systematic training in forestry than was then available. The importance of the conservation of the forests of the country, and of their extension, both for the sake of amenity and economy, were prominently brought out by the Exhibition itself, by the various papers connected with it, and by several lectures he was instrumental in appending. By his influence the Board of Agriculture agreed to give a sum of £100 per annum, and the Highland and Agricultural Society £50 per annum, while the sum of £1000, then anonymously given, most generously came from himself—all to found a Chair of Forestry in the University of Edinburgh, a project he had kept in view for some years. He succeeded in a way which only high purpose and skilful diplomacy could, and now a Forestry Lectureship is permanently established. The first lecturer was Dr William Somerville, who now holds the Professorship at Durham, and the present holder is Col. Bailey, R.E., who was lately in charge of the Forest School of the N.W. Provinces of India. The gratitude of all students of forestry was thus worthily won by his indefatigable exertions and his generous help. It is satisfactory to know that his botanical library also goes to the University of Edinburgh.

Further, besides extending the interests of the Department in London and Edinburgh, he did not forget the efforts that were then being made in the University of St Andrews to widen the sphere of teaching, especially by the introduction of Botany into the curriculum. His counsel and encouragement were always at the service of those working in this direction. Accordingly, when lectures on botany were commenced in 1887, he most kindly gave numerous lecture-diagrams, a botanical cabinet and herbarium, a series of reference-books, and took a personal interest in the success of the class, then under Dr J. H. Wilson, now of the Yorkshire College, Leeds. He also gave prizes to the best students, and invited them to Stravithie to botanise, and in various ways encouraged the new lectureship. A year or two afterwards he intimated that he intended to give £1000 to the Chair of Natural History in St Andrews, formerly held by his relative, Prof. Hugh Cleghorn. The struggling lectureship in Botany, however, was more in need of it, and he was advised to make it secure by this handsome donation. He agreed; and the necessary document was there and then drafted, at that time anonymously. In the formation of a Botanic Garden in the University he took a deep interest, and spoke at the opening ceremony in the Garden in the summer of 1888, especially dwelling on the practical skill which the young lecturer (Dr Wilson) had shown in arranging the natural orders of plants.

Dr Cleghorn's distinguished career in India, his unselfish devotion to the good of the community, and his active efforts to advance science—especially in the two Universities with which he was more immediately connected—did not pass unnoticed. In 1885 the University of St Andrews bestowed on him the honorary degree of Doctor of Laws. He was made an Honorary Fellow of the Botanical Society, a distinction limited to six British subjects; and he was elected also an Honorary Member of the Scottish Arboricultural Society, and a Vice-President of the Literary and Philosophical Society of St Andrews,—his last appearance having been made before the latter body, viz., in reading a paper by Mr Coldstream on Fruit-growing in India, a subject he had long advocated. He was also elected Assessor to the University Court of the University of St Andrews by the General Council, and he held this office with great acceptance till failing health necessitated his retirement—to the regret of his friends in the University. Finally, at a meeting of the Scottish Arboricultural Society in 1888, he was presented with his portrait, subscribed for by all classes, from peers to foresters and horticulturists, and a sum of £200, which latter he suggested should go to form a forest-library. On this occasion a graceful tribute was paid by Principal Sir William Muir to Dr Cleghorn's services to science, and to arboriculture in India and in this country; and he concluded by announcing that the "Hugh Cleghorn Forest-Library" would be placed in the Museum of Science and Art, Edinburgh. Nothing, in short, could have been more agreeable to the recipient, or more honourable to the large number of distinguished men who had subscribed, and some of whom were present.

The career of Dr Cleghorn was singularly varied, but throughout there runs the thread of a true devotion to science, especially to botany and forestry. His services to the State in India and at home, in the cause of the latter, have been noteworthy, and will long remain to testify to his perseverance, his breadth of view, and his great powers of observation. As a philanthropist and educationalist, again, he played an important part—in fostering all that was good in life on the one hand; and on the other, in giving substantial encouragement to science in the Universities, and in aiding them to extend their influence amongst the people.

He was a man of high personal character, a genuine Christian, and a generous landlord. His genial and kindly bearing everywhere gained him esteem. For some time he had been in failing health, and in early spring he somewhat suddenly broke down, peacefully passing away in his quiet home at Stravithie, mourned by all around him, and by a wide circle of friends elsewhere.

## Thomas Stevenson, C.E. By Professor Swan.

(Read July 15, 1895.)

Thomas Stevenson was born in Edinburgh on the 22nd July 1818. His and my own grandfather was Thomas Smith, the first engineer to the Board of Northern Lights; and his father was Robert Stevenson, who succeeded Mr Smith in that office, which he held for the long period of forty-five years, and who was the designer of the Bell Rock Lighthouse. This tower, built under his personal superintendence before the days of steam navigation, on a low-lying reef continually submerged during neap-tides, was a work of peculiar difficulty, and its successful accomplishment an achievement probably not yet surpassed in lighthouse construction.

Thomas Stevenson and I received our first schooling from Alexander Brown, then well known in Edinburgh as a highly-accomplished teacher of English, but who was also an exceedingly severe disciplinarian. Neither of us suffered much from Brown's tawse, even although they were not only in continual use in inflicting *palmies*, but also not unfrequently in what might be termed unlimited flogging. But Tom had rather frequent experience of being "kept in" after school hours, on account of lessons imperfectly prepared; and, in years long after the times of Brown and of the High School of Edinburgh, might be heard to say that none of the troubles and trials of his manhood were so hard to bear as the sufferings he had endured at school. School discipline in those days was indeed too often a system of merest terrorism and repression, or repressive terrorism. I have a lively recollection when a book, then familiarly known as *Ruddiman's Latin Rudiments*, was first put into my hands, how I had a strong desire to discover whether any occult relation subsisted between the words "Ruddiman" and "Rudiments." But I dared not ask my tutor, to whom putting questions was a crime, and who would have ordered me reprovingly to hold my tongue and mind my lesson.

From Brown's school Tom proceeded to the High School of

Edinburgh, where he received the education in classics which that institution afforded. Throughout, like many others of its scholars who in after life have distinguished themselves, his position was at best that of respectable mediocrity. Yet he succeeded in accomplishing what, in those days at least, was not so very common. He carried away with him some knowledge of Latin, which he was able to revive and heartily enjoy in later years. A predilection for letters, although doubtless inherited, was, in spite of his stern discipline, evoked into conscious activity by his master, Alexander Brown, and must have received further development at the High School; for, from an early period, he displayed a noteworthy love of books, and early became an ardent book collector. In these days it was far easier than it now is to pick up old books at moderate prices; and, even within the very limited means placed at his disposal, he evinced much good taste and judgment in his purchases. Among these was a copy of *Æsop's Fables* with Bewick's woodcuts, now in my possession. But his chief triumph was falling in with a copy of the excessively rare *Croniclis of Scotland* of Hector Boece (Edinburgh, 1527).

In default of becoming an engineer like his father—an aspiration which as yet had received no encouragement—it was but a natural outcome of his love of letters, and still more of their material embodiment in the shape of books, that he should now have contemplated the calling of a printer as a possible alternative. In accordance with such prospects, about this time he had got made for himself a working model of a printing press. This was not a mere toy, but a regular "Columbian" press; and his notion was to write essays of his own, and to print them. I remember one fragment which thus went through the press. It was written in typical Johnsonian English, due to the inspiration of *Rassellas, Prince of Abyssinia*, and other writings of the great lexicographer, caught under the contagious enthusiasm of Alexander Brown. But a ready use of his fingers, unless it were in writing, was not one of Tom's natural gifts, and the work of compositor and pressman was speedily abandoned.

But now it was time that he were choosing a calling for life. His tastes and aspirations were for his father's profession. But already his elder brothers, Alan and David, had adopted it; and it seemed as if there were not room left for Tom. Accordingly he chose, at least provisionally, what he liked next best, and was received, without a regular apprenticeship, into the printing-office of Dr Patrick Neill, his father's much esteemed and life-long friend. Here he found, I believe, sufficiently congenial employment, and the days went pleasantly by, but varied by, at least, one sufficiently startling experience. One morning when on his way to the printing-office in the Old Fishmarket Close, when it was blowing a furious gale of wind, a heavy chimney-pot from some of the lofty High Street *lands* fell so close to his feet that a fragment of it rebounding from the pavement, cut his dress, but happily without striking his body; and thus it may be said that his temporary typographical pursuits had in more ways than one nearly lost him for his great life's work as an engineer. Now, however, he entered his father's office before he had completed his eighteenth year.

He afterwards superintended the construction of various works, among which was the lighthouse on Little Ross Island; and it was while thus engaged that he wrote a paper on the geology of that island. This was published in 1843; but a still earlier paper, which appeared in 1842, "On the Defects of Rain-gauges, with the description of one of an improved form," was the first of a series of writings, in course of time, contributed to various scientific societies and journals embracing a very wide field, and which included lighthouse and harbour engineering, lighthouse optics, experiments on the force of waves, meteorology, and other subjects. In 1846 he became a partner with his brothers Alan and David, of whom the former, who had succeeded his father as engineer to the Board of Northern Lights in 1843, in 1853, owing to ill-health, resigned that post, when his brother David was appointed in his place as engineer to the Board, and in 1855 Thomas was conjoined with David in the engineership. During their joint tenure of office, extending over a period of thirty-two years, they designed and erected numerous beacons and lighthouses, among which the lighthouses on Dhu Heartach and the Chicken's Rocks were works of no ordinary difficulty. During his brief term of active professional work, Alan Stevenson had designed and personally superintended the erection of the magnificent lighthouse tower on the Skerryvore Reef, and had also introduced into the Scottish lighthouse system Fresnel's dioptric apparatus, on the construction of which he made some important improvements, among which were the employment of diagonal in place of rectangular joints in Fresnel's refractory apparatus, and with the same object—namely, of obtaining more uniform distribution of light—diagonal astragals in the lighthouse lanterns. With a like spirit his successors remodelled many of the earlier Scottish lighthouses, replacing the older reflector apparatus by the lenses and other agents of the dioptric system; and it was in this work that Thomas achieved the well-earned and world-wide reputation of being first in his time in the improvement of lighthouse apparatus, "by whose devices," it has with literal truth been said, "the great sea-lights in every quarter of the world now shine more brightly."

Augustin Fresnel (B. 1788), in June 1819 placed by the Government of France on the Commission des Phares, in August of the same year submitted to the Commission his design of a polyzonal lens, which he proposed should supersede the reflectors then in use in the lighthouses of France. Owing to the then imperfect condition of the art of glass-working, difficulties were at first experienced in the manufacture of these lenses, which were only overcome by new methods of Fresnel's own devising; and it was not until July 1823 that the famous old lighthouse tower at Cordouan, where it had been determined to inaugurate the new system of lighthouse illumination, was lit up by means of lenses in place of the reflectors which had served since 1799.

Meanwhile Fresnel had communicated to the Academy of Sciences, on 29th July 1822, his *Memoire sur un nouveau systeme d'eclairage des Phares.* This, the only considerable published writing on his "new lighthouse system" proceeding from his own pen, consists in a description of his polyzonal lenses and their arrangement for revolving lighthouse apparatus, where, in order to save light which would otherwise be lost by upward divergence, each large lens is accompanied by a smaller one placed above in connection with an inclined mirror, the effect of the arrangement being to increase the duration of the flash from the great lens.

But he died in 1827, and thus but the five remaining years of his brief term of life were afforded him in which to work out his dioptric system of lighthouse illumination; but in these years the optical agents which he had devised—each perfect of its kind—were two in number,—his polyzonal lens, in which the centre and radius of curvature for each zone are separately computed, so as almost entirely to get rid of spherical aberration, and his totally reflecting lighthouse prisms. But the application of these agents to the design of his fixed light apparatus he did not live to see actually constructed in all the details in which he had conceived them. It was not until further experience in the manufacture of lighthouse apparatus had rendered it practicable that his designs were for the first time carried out in Scotland by Alan Stevenson, and then, as at length constructed after what we cannot doubt was Fresnel's own ideal design, his fixed light apparatus may be regarded as a thing perfect of its kind. Similarly, his revolving light arrangements, as they left his hands, more especially when viewed in the light of subsequent improvements, wear the aspect of being but a first attempt; and, accordingly, Alan Stevenson when contemplating the arrangements to be made for his lighthouse of Skerryvore, dissatisfied with those of Fresnel, introduced the substantial improvement of adding fixed prisms below Fresnel's revolving lenses, these Fresnel-prisms, manufactured in Paris, being the first ever constructed for the large dimensions of a first-order light. Had Fresnel's life been prolonged, there is every reason to believe that the dioptric system of lighthouse illumination would have received further development at his own hand; but now, when that hand was for ever still, the distinction of being his chief successor in the work of improving lighthouse optical apparatus did not fall to any one among his own countrymen, but was reserved for Thomas Stevenson, the subject of this memoir.

This life's work, judging from the date of his earliest publication on the subject, may be said to have begun in 1849, even before his appointment as one of the engineers to the Board of Northern Lights, and to have been continued, so long as health permitted, up to within a few years of his death in 1887. Always on the outlook for improvements, and more especially while under the fresh stimulus arising out of the erection of some new lighthouse, his singularly active mind was continually suggesting something new. In the course of years his inventions, greater and smaller, arising in this way became so numerous that it is scarcely possible in a brief notice such as the present to give any really adequate account of them, and for the best available information regarding them reference may be made, once for all, to his *Treatise on Lighthouse Construction and Illumination*, of which the third edition was published in 1881. Yet the history of inventions has seldom failed to be interesting to a numerous class of readers; and here it seems desirable at least to point, in chronological order, to Thomas Stevenson's principal lighthouse improvements.

Of these the earliest, made in 1849–50, was the invention of his Catadioptric Holophote. This consisted in the removal of the posterior portion of the parabolic reflector hitherto in use in lighthouses, replacing it by a spherical reflector behind and concentric with the flame, so as to reflect the light incident on it back through the flame along with a lens in front, by which means he contrived that, while by far the greater portion of the original parabolic mirror was retained to fulfil its function of reflecting a beam of parallel rays, the very large portion of light which hitherto had wastefully escaped by natural divergence, was now also emitted in the same beam of parallel rays with that reflected by the parabolic surface. Thus, for the first time, the whole "sphere of rays" diverging from the flame was utilised by being combined into a single beam of parallel rays, with the least possible number of reflections or refractions. The merit of this contrivance will best be appreciated by comparing it with previous attempts made to increase the efficiency of the parabolic lighthouse reflector. It is the first of various arrangements which Mr Stevenson devised in order to intercept, and by the least possible number of optical agents to render parallel, *all* the rays proceeding from a focal point, and which accordingly he termed "holophotal."

In designing a lighthouse where a portion only of the horizon and not the whole all round was to be lit up, the catoptric system of illumination presented no difficulty. It was enough to fit up a system of reflectors embracing in their range the arc to be illuminated and no more. But it is otherwise with the dioptric system. A portion only, instead of the entire Fresnel's fixed-light apparatus, doubtless, can be constructed. But then those rays proceeding from its great central lamp, amounting it may be to more than one-half, which this partial apparatus fails to intercept, will be uselessly scattered in landward directions—never, indeed, escaping from the lantern, which in that aspect is always constructed of metal or other opaque material. It was to save, as far as possible, this intolerable waste that Alan Stevenson, in Sanda Island Lighthouse, which was lighted in 1850, introduced a large spherical mirror of silvered glass of, I believe, 5 feet 10 inches radius, concentric with the flame, so as to *reflect back through the flame* the light which otherwise would have been wasted. It is obvious that rays thus reflected back will proceed forward thereafter *from the flame*, just as if they had been emitted directly by it, and thus they will fall on the optical apparatus in front in such directions only as will enable it to transmit them to the seaward horizon. Whether this method of reflecting light back through the flame was thus for the first time employed in lighthouse engineering, I am not aware; but Mr Stevenson has stated (*Account of the Skerryvore Lighthouse*, p. 293) that it had been originally suggested by him so far back as 1834.* We have already seen that this device was also in 1849–50 adopted by Mr Thomas Stevenson in the construction of his Catadroptric Holophote, of which it forms an essential element. He, however, for the purpose of returning back light through the flame did not long rest contented with ordinary mirror-reflection. Having accidentally noticed the brilliant specular look of light emerging from a glass prism, where it had undergone two internal *total* reflections, he conceived the idea of constructing the spherical mirror in his holophotal apparatus of such prisms; and, accordingly, having consulted me, I assigned their proper form and mode of combining them, so as to form a totally reflecting hemisphere (*R.S.S.A. Trans.*, 1850, vol. iv. p. 20). But it was only at a considerably later period that the first complete catadroptric mirror was constructed for the Commissioners of Northern Lights by Mr J. T. Chance, and it was shown by them at the London International Exhibition of 1862. For this instrument Mr Chance devised a new arrangement of the prismatic zones, which greatly facilitated its construction. Their figures were now generated round a vertical instead of a horizontal axis as formerly, and they no longer formed a continuous hemisphere, but were separated from each other. Having thus devised the means of

* This was in a Report to the Commissioners of Northern Lights, 10th December 1834, p. 28. Edinburgh: Printed by Neill & Co., 1835.

reflecting the "back" rays of the flame in his holophotal apparatus arrangements by dioptric agency to the exclusion of metallic reflection, the next problem which suggested itself to Mr Stevenson was how to obtain a dioptric arrangement which should be capable of dealing with *the whole* of the front rays, hitherto in part subjected to metallic reflection. A Fresnel polyzonal lens had, indeed, received the central pencil of these rays, diverging all round the horizontal axis of the lens through an angle of about 45°, which, after refraction, were emitted in directions parallel to that axis. Beyond some such limit of divergence, varying with the kind of glass of which the lens was composed, mere lenticular action was unavailing towards emitting a parallel beam of rays. But now it occurred to Mr Stevenson that the agency of which he was in quest was to be found in Fresnel's catadioptric zones, provided they were generated by revolution round a horizontal instead of a vertical axis; and thus, by means of a Fresnel polyzonal lens surrounded by a series of totally reflecting rings, generated in the manner now described in front and with a totally reflecting dioptric mirror behind, Mr Stevenson obtained a holophote, in which, instead of the optical agents being, as in his earlier invention, partly metallic reflectors and partly lenticular, *all* were now dioptric.

Zones of glass the same in section with those in Fresnel's fixed light, but generated by the revolution of their section about a horizontal instead of a vertical axis, which he had thus devised in order to complete his dioptric holophote, he not unnaturally termed "holophotal."* The two species of zone, although identically the same in section yet differing in their mode of generation, consequently differ in their optical effect. Light from the focal point incident on a Fresnel's fixed-light zone is emitted in every azimuth, but all within a horizontal plane. Light similarly incident on the so-called "holophotal" zone is all emitted in one and the same direction parallel to the axis of revolution of the zone, so that its action is identical with that of a lens in rendering parallel rays proceeding from its principal focus, but capable of producing much greater deviations than lie within the power of any lens. Lenticular action was thus extended from causing deviations from about 45°—the limit of Fresnel's polyzonal lens—to about 130°.

* *Lighthouse Construction and Illumination*, 1881, pp. 83–85.

But it is proper here to mention that zones of this description are said to have been constructed for Augustin Fresnel so long ago as 1826. Yet it would seem that no drawing or description of them was ever published, nor were they ever used in any lighthouse. If they had existed at that early date, there is evidence to prove that they had been forgotten; nor can there be any doubt that they were independently invented by Mr Stevenson.

And now that he had obtained for himself this new and powerful auxiliary, he forthwith proceeded to apply it to the improvement of Fresnel's revolving-light apparatus. This had hitherto consisted in a system of his great annular lenses surrounding the central burner, the light which by upward divergence escaped from their action, and would have been lost, being received on a combination of smaller inclined lenses, which transmit it to plane mirrors overhead, and these finally reflect it outwards towards the horizon, to strengthen the light emitted by the great lenses. Now Mr Stevenson discarded the complex arrangement of the smaller lenses and plane mirrors, replacing it simply by a system of his new holophotal prisms. This capital improvement was first introduced in North Ronaldshay Lighthouse in Orkney, for which the new prisms were made in 1851 by M. Letourneau of Paris. Unquestionably it effected a great saving of light. Fresnel himself, in his *Mémoire* of 1822, had estimated that the loss of the light subjected to the successive action of his lenses and plane mirrors amounted to one-half; while Mr J. T. Chance remarks that in Fresnel's revolving apparatus, as the focal distance of the accessory lenses is less than one-half of the shortest focal distance in the system of reflecting zones, the intensity of the light issuing from the former would be scarcely more than one-fourth of that transmitted by the latter; and, in addition to this cause of inferiority, is the loss arising at the mirrors, so that, on the whole, the modern plan (holophotal) must give light *five* or *six times* more intense than that of the former (Fresnel's) arrangement.

It must suffice here simply to mention that, just as in the case of "Fresnel's revolving light," so also in his "fixed light varied by flashes," similar improvements were effected by Mr Stevenson by substituting holophotal prisms in place of the two-fold refracting and reflecting agents employed in the original apparatus; and it must suffice also merely to name his "Improved Dioptric Holophote" of 1864 before passing to the consideration of his "Azimuthal Condensing System of Lighthouse Illumination." This, which may be regarded as his crowning achievement in improving on the former condition of lighthouse optical engineering, occupied his attention for a period of fully thirty years (say from 1855 to 1885), during which time he expended on it quite a wealth of inventive faculty, and in the end may be said to have brought about a total revolution in lighthouse construction.

He himself had not exceeded the literal truth when he wrote that "previous to 1855 lighthouse apparatus, having the same illuminating power in every azimuth, was used not only at places where the distances from which the light could be seen were everywhere equal, and where the employment of such apparatus was therefore quite legitimate, but also for places having a searange much greater in some directions than others. This indiscriminate application of apparatus of equal power to the illumination of our coasts necessarily involved a violation of economic principle, for the light was either too weak in one direction or else unnecessarily strong in another." "In other cases, where perhaps only half the horizon had to be lighted, a single flame in the focus of a fixed apparatus could also be strengthened by a hemispheric reflector placed on the side next the land." "But no attempt was ever made to allocate this auxiliary light in proportion to the varying lengths of the different ranges and the amplitudes of the arcs to be illuminated; nor, where a light had to show all round the horizon, to weaken its intensity in one arc, and with the rays so abstracted to strengthen some other arc, which, from its range being longer, required to be of greater power. As none of the agents or combinations which we have as yet described were sufficient for dealing with this branch of lighthouse optics, I found it necessary to devise eight new agents, possessing special optical properties, for distributing the rays not *equally* but equitably" (*Lighthouse Construction and Illumination*, 1881, pp. 97–8).

Some, at least, of these new agents will now fall to be described, and of these the first, taken in the order in which Mr Stevenson has described them, is that which he has termed a "back prism" (*op. cit.*, p. 91).

Fresnel determined the angle subtending the reflecting side of the lighthouse prism by the condition that the paths of the extreme rays of the intromitted pencil of light should be respectively parallel to the sides containing that angle. This construction, although elegant, is not essential to the action of the prism, and is even disadvantageous, as causing the loss of excentric rays, so that the angle of the prism might with advantage be diminished to the needed amount. But another disadvantage of Fresnel's construction is this, that while with prisms so devised by him deviations of light cannot be obtained exceeding from about 100° to 120° according to the kind of glass employed, by freeing ourselves from the limitation which his construction imposes we can quite advantageously obtain deviations up to 120° or 135°. The utility of this in the construction of apparatus to deal with what has been termed "the back light" is obvious; and, accordingly, Mr Stevenson states that the late Mr Alan Brebner and himself had designed what they termed "back prisms," by which rays may be made to deviate from their original direction by about 130°; and he adds, "I communicated the description of these prisms to the Royal Scottish Society of Arts on 6th December 1867.* Professor Swan of St Andrews also independently proposed the same form of prism, a description of which he communicated to the same Society on the 9th December 1867, accompanied by general formulæ for its construction."† These prisms were first used at Lochindal lighthouse in Islay, and were made by Messrs Chance in accordance with Professor Swan's formulæ.

The next of Mr Stevenson's new agents, devised by him in carrying out his azimuthal condenser system of lighthouse illumination, which falls to be described is his Differential Lens. The action of this instrument may best be understood by considering first that of a Fresnel's Polyzonal Lens with the source of illumination in its principal focus. The light diverging from the flame and falling on the plane surface of the lens will, after refraction, be emitted in a beam of parallel, and, it may be assumed, horizontal rays. But if now for the plane face of the lens be substituted a cylindric surface

* *Trans. Roy. Scot. Soc. Arts*, vol. vii., 1868, pp. 540–546.

† This modification was independently suggested by Mr Chance and by myself.

whose axis is vertical, and which therefore is capable of refracting light in horizontal directions only, it is evident that the rays now emitted by the lens will still emerge in horizontal directions, but now no longer parallel to each other, but diverging each from some point of a vertical focal line whose length is equal to the diameter of the lens. The angle of divergence, it is evident, may be adjusted to any required amount by varying the curvature of the cylindrical face of the lense, and just as the central disc and sun surrounding lenticular rings of the Fresnel polyzonal arrangement replaces a simple plano-convex lens; so, in place of a single cylindrical surface for the differential lens, may be substituted a central cylindrical band bordered on either side by a series of straight lenticular prisms with vertical arcs.

Mr Stevenson's Differential Refractor is the application of the same principle which has been described above for the lens to Fresnel's Cylindrical Refractor; and, for like ends, Mr Stevenson also devised a Differential Refractor, of which, in his own words, "the vertical section must be parabolic, while in the horizontal it must be of such hyperbolic, elliptic, or other curve as will most advantageously give in each case the required horizontal divergence."

Professor Tait was kind enough to investigate the mathematical conditions of the differential mirror, and in the *Proceedings of the Royal Society of Edinburgh* of 1871, he gives by a quaternion integration the formulæ for its construction. For a description of the remainder of Mr Stevenson's new optical condensing agents it must suffice to refer the reader to the descriptions to be found in his *Treatise on Lighthouse Illumination.*

The preceding statement, I believe, will be found to include a tolerably complete enumeration of Mr Stevenson's inventions in lighthouse optics; but also nothing as yet has been said regarding the application of these to special cases of lighthouse illumination. Many such there are of very great interest; but it is impossible to include any adequate description of them within the limits of the present notice. It must suffice, then, simply to point to one, namely, the Isle Oronsay Light, situated in the narrow and tortuous Sound of Skye. This light, according to the direction from which it is viewed, is visible at very different distances, varying from 3 to 15 miles. Accordingly, it became an object of importance to distribute the light supplied by an ordinary Fresnel's second-order fixed apparatus, in the various directions in which it was to be viewed in quantity in some measure proportional to the distance it would have to travel to reach the observer's eye, and this was effected by subjecting portions of the light not otherwise usefully available to the action of condensing prisms (*Lighthouse Illumination*, pp. 112–116). The late Mr James Melville Balfour, in reporting on the first trial of this light (October 1857), says, "The prisms throw a light down Sleat Sound superior to any first-class light in the Northern Light's service, and the light up Glenelg Bay is little, if at all, inferior in power" (*op. cit.*, p. 116).

Here it falls to be recorded that Mr J. M. Balfour not only had the charge of the erection of the Isle Oronsay Light, but to him also was committed the working out of the necessary drawings and calculations required in designing it. In this work he obtained invaluable, it might indeed be almost said essential, help from a recent ingenious invention of his own. This was his "Optical Protractor," the first instrument, I believe, of its kind (described in the *Transactions of the Royal Scottish Society of Arts*, vol. v.).*

And here seems a fitting opportunity for remarking how fortunate, in carrying out the application of his new lighthouse agents to actual lighthouse construction, Mr Stevenson was in possessing in the firm's office two such coadjutors as Mr J. M. Balfour and Mr Alan Brebner. The latter, who became a partner in the firm of D. & T. Stevenson, C.E., and died in 1890, was the inventor of a new optical protractor differing from Mr Balfour's. During a period of many years he, along with other assistants in the office, executed the designs for the lighthouses constructed by the firm. Of the value of their co-operation in the work it is impossible to speak too highly.

There still remain unmentioned many of Mr Stevenson's lighthouse inventions, which, being not *purely* optical, have not been included in the preceding enumeration, but space can only now be

* I have elsewhere (in my paper "On New Forms of Lighthouse Apparatus," *Trans. Roy. Scot. Soc. Arts*) expressed my extreme obligation to Mr Balfour for the invaluable aid I had obtained from the use of his ingenious instrument, without whose help I should scarcely have undertaken to protract the designs contained in my paper.

found to mention but one or two of these. The first I will name is that which he has termed "The Apparent Light." This was first designed by him for Stornoway harbour, and erected in 1851. This harbour has a very narrow entrance, whose available width is still further reduced by the presence of a submerged reef. To build a lighthouse on this reef would have been a very costly undertaking, but Mr Stevenson contrived to light up the hidden danger in another way, by availing himself of the already existing lighthouse on Arnish Point. In a window near the bottom of its tower he placed a lens capable of transmitting a horizontal beam of parallel rays to a lantern carried on the top of an iron beacon 25 feet high, which he built on the submerged reef. There the rays are received on a system of vertical prisms, which disperse them seaward over an angle of 62°. It is scarcely necessary to add that the light thus dispersed will appear to the mariner as if it proceeded from an actual lighthouse built on the submerged rock, and not from a lamp placed on the distant shore.

To this enumeration of Mr Stevenson's very varied achievements in lighthouse illumination, I will add the success which crowned his experiments on illuminating beacon or buoys by means of electricity conveyed from land through submarine cables, as fully realised in the lighting of Gedney's Channel, leading to New York Harbour.

Mr Stevenson was a Member of Council of the Scottish Meteorological Society from the time the Society was established in 1855, and its Honorary Secretary from 1871 to his death in 1887. During this long period he not only took an active and earnest part in the management of the Society's affairs, but also made original and permanent contributions to the science of Meteorology. Of these contributions the most important are these:—

1. The Stevenson Screen for the protection of Thermometers, designed by him in 1864. The object sought to be obtained was UNIFORMITY among temperature observations, and in this he succeeded so largely that the Stevenson Screen quickly came into use, and continues to be used extensively in all parts of the world.

2. The introduction into meteorological investigations of the term "Barometric Gradient" in 1867, which he first applied, and with great success, in a discussion of the facts of our great Edinburgh hurricane of January 1868.

3. An elaborate inquiry, with numerous anemometers placed on poles at various heights, into the important question of the rate of diminution of the wind's velocity with height.

4. The proposal, in 1875, to obtain from High Level Stations and Observatories the data for determining "Vertical Gradients" for atmospheric pressure, temperature, and humidity, from their important bearing on meteorological questions generally, but more particularly on weather changes. Ben Nevis Observatory and High Level Observatories of other countries have now been for some time carrying on this great work in a degree and to an extent scarcely in contemplation when the proposal was made in 1875.

The Council of the Society recorded in the Minutes of their meeting of July 11, 1887, their grateful testimony to the vigour and prudence with which Mr Stevenson discharged his duties as Honorary Secretary, and to his unfailing courtesy and readiness to aid and oblige on all occasions.

(*Supplementary*, by D. A. Stevenson, Esq.).

In addition to what has already been stated, should be mentioned his design of a Bivalve apparatus in 1859; also the design and introduction, in 1886, of "Lightning Light" apparatus at the Isle of May electric light, both of which have been strongly advocated since by the engineers of the French Lighthouse Board. But, perhaps, the most notable improvement of recent times is his firm's design of the Hyper-radiant apparatus. This apparatus was designed to take advantage of burners of increased diameter and initial power, and, when tried at the South Foreland in 1885, it was found to be greatly superior to the other lenses tried against it, and is now largely used at home and abroad.

Along with his brother David he made experiments in 1870 on the use of paraffin as a lighthouse illuminant, and after trial at Girdleness Lighthouse, they reported to the Commissioners of Northern Lighthouses that paraffin should be introduced into all the Scottish lighthouses, as its use would increase the intensity of the lights, while their maintenance would be greatly reduced.

Paraffin is now almost universally used in lighthouses. At present prices, and at the present rate of consumption, the use of paraffin in British lighthouses alone results in a yearly saving of between £35,000 to £40,000.

His firm's practice as lighthouse engineers was not confined to the Scottish Board, as their advice was taken by the Governments of India, China, Newfoundland, New Zealand, Japan, and other foreign governments, and schemes for the lighting of the whole coasts of the two last named countries were devised and are now being carried out. In his book on *Lighthouse Construction and Illumination*, the results of the practice of his firm in lighthouse construction and optics are given. For some of his inventions laid before the Royal Scottish Society of Arts, Mr Stevenson was awarded gold medals; but it is perhaps proper to say that for none of his inventions has he, or any member of the Stevenson family, taken out patents, all lighthouse authorities having had free use of their designs and improvements in dioptric apparatus and lighthouse economy generally.

Perhaps I may be allowed to quote the testimony of Captain Sulivan, the professional adviser of the Board of Trade, when in 1861, giving evidence before the Royal Commission on Lighthouses, he stated:—"It is to Mr Stevenson we owe the present state of our lighthouse illumination—for the improvements on the Fresnel light which he has made have really given us the superior class of lights that we have now in England. All that has been done, that I can see, to improve on the system, and to give us a better class of dioptric light, has been done by Messrs Stevenson, and I believe that that is quite the feeling of every one at the Board of Trade."

The practice of the firm, of which Mr Thomas Stevenson was a member, was not confined to lighthouse engineering, as they were mainly engaged in the construction of harbours, docks, and river and estuary improvements. With most rivers and harbours in Scotland he and his partners were in some way professionally connected, while they were also called upon to design works for the improvement of harbours, and of many rivers and estuaries in England and Ireland. To the subject of harbour construction Mr Stevenson directed his attention, bestowing special care in ascertaining the forces which have to be met and overcome in the erection of works exposed to heavy seas in deep water. He devoted special care to ascertain the force, height, and laws of the propagation of sea waves, and their action on artificial structures. The measurement of the force of the waves was carried out with some degree of completeness by means of instruments which he devised, such as the Marine Dynamometer. The result of his wave observations, and the laws he deduced from them, were given in papers communicated to this Society, to the *Edinburgh Philosophical Journal*, and also in his book on *Harbours*. In 1852, after a series of experiments, he enunciated the law of the increase of the height of waves in relation to "fetch"; other experiments led to formulæ involving the relations between the heights of waves and the various influences which modify them, and also to formulæ by which the reductive powers of harbours and breakwaters—or their power of reducing the height of waves after passing within the entrance—could be calculated, all of which are of great value to the marine engineer. He always held, however, that much remained to be done, especially in ascertaining facts, and he considered his own work in this direction as only approximations. More than thirty years ago he was invited to write the article "Harbours" for the *Encyclopædia Britannica*, which was subsequently published as a separate treatise on *The Design and Construction of Harbours*, and it is now in its third edition.

He came of a well-known family of engineers, all of whom have been highly distinguished in their profession; and his nephew, David Alan Stevenson, was conjoined with him in the engineership of the Scottish lighthouses on his father's resignation, and is now the Engineer of the Board, being the fifth in succession in that office.

He was elected a Fellow of this Society in 1848, acted as a member of Council, as one of its Vice-Presidents, and in 1885 was elected its President. He frequently contributed to our *Proceedings*. He was elected a Member of the Institution of Civil Engineers in 1864; and one of his last literary works was a lecture on "Tides and Coast Works," which he prepared at the request of the Council of that Institution, but which, owing to ill health, he was unable to read. He was a Fellow of the Society of Antiquaries of Scotland, the Geological, and other Societies.

Mr Stevenson was a devoted member of the Church of Scotland. Under the pseudonym of a "Layman," he wrote several pamphlets on religious questions, one of which was reprinted by the late Professor Crawford for the use of his students. He did much in unostentatious charity, many institutions finding in him a warm and generous supporter.

Mr Stevenson was incapacitated for business for a short time previous to his death. He spent the winter of 1886–87 with his late son, Mr R. L. Stevenson, at Bournemouth, returning to Edinburgh only a fortnight before his death, which happened on the 8th May 1887.

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